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INSTINCTIVE BEHAVIOUR AND ENJOYMENT.

BY C. LLOYD MORGAN.

(1) *Its character as such* (pp. 1–3).
(2) *Its dependence on heredity* (pp. 3–6).
(3) *Concrete instances of behaviour seldom purely instinctive* (pp. 6–8).
(4) *Instinctive enjoyment correlated with instinctive behaviour* (pp. 8–10).
(5) *Is there instinctive knowledge and prevision* (pp. 10–12)?
(6) *The relation of instinct to intelligence in the individual* (pp. 12–16).
(7) *And in the race* (pp. 16–18).
(8) *Levels of complexity. Matter and form* (pp. 18–20).
(9) *Nature, disposition, inclination, and impulse* (pp. 20–22).
(10) *Disposition as* ad hoc *preparedness* (pp. 22–25).
(11) *Innate tendencies in human life* (pp. 25–28).
(12) *Conclusion* (pp. 28–30).

(1) The word 'instinctive' is one which an author feels free to use in such wise as shall best serve his purpose in hand. There is no established convention in accordance with which its connotation is definitely settled. There is not even agreement as to that to which the word is to be taken as adjectival. Some speak of instinctive impulse; others of instinctive dispositions; yet others of instinctive knowledge, instinctive appreciation, instinctive belief, and so on.

Without prejudice to other applications, the word 'instinctive' may unquestionably, in accordance with good precedent, be applied to behaviour. Let us take this as our point of departure. The advantage of doing so is that behaviour is something that can be observed—the life-history of which can be traced. This serves to indicate that what we are here to start with is that which we *can* thus observe—that which *is* actually given for our consideration and interpretation. If it be asked what it is that behaves, our reply here is that it is an organism, including of course man—not an organ or a part of an organism, though these are included, but the organism as such and as a whole. It is clear therefore that our departure platform is to be frankly biological. This it is meant to be. That does not imply however that it is to be biological only. I assume throughout that instinctive behaviour has an inner aspect of instinctive enjoyment—that, in a broad sense of the word, it is conscious behaviour with which we are to deal. Just as the behaviour we observe is an integral phase in the life-history of the organism, so is enjoyment in so behaving an integral phase in the life-history of the psychical system correlated with functional processes in that organism. For biological treatment, therefore, our departure platform is instinctive behaviour. For psychological treatment it is the instinctive enjoyment which is the psychical correlate of such behaviour. I borrow the word 'enjoyment' from Prof. Alexander; but I use the word 'instinctive' with a different connotation.

Now we may distinguish the ground and the conditions of behaviour. The ground, as I use the word, is the intrinsic nature of the organism concerned. The conditions (extrinsic) are afforded by the situation in which that organism is placed. We speak of the effective influence o the total situation as affording the stimulation under which the responsi behaviour is evoked. But we must remember that the nature of t organism, under its existing poise at the time of stimulation, is the groun of action, in the absence of which there would be no such response. Fo purposes of interpretation, however, we have also to consider the observable outcome of the behaviour, what it leads to, what it is good for. Thus we have the instinctive behaviour itself; its ground in the nature of the organism; the conditions under which it is evoked; and its utility or prospective value.

Under the instinctive behaviour itself fall multifarious instances of thus behaving. It is the group-name under which they are all comprised. There would be no point, however, in using the distinguishing adjective, if there were no characters which earmark any given instance of thus behaving. What, then, are the criteria of behaviour as instinctive, by which all instances which have them may be distinguished from other instances of behaving? Can we say from simple inspection whether some observed act of some given animal or of man is instinctive or not? We cannot do so. There is nothing on the face of it to enable us to say whether it is or is not. We must broaden our view so as to assign to it a place in the history of the organism. Now here the criterion of instinctive behaviour which has been widely accepted, and is here accepted, is that the act which affords an instance has not been learnt or acquired in the course of individual life. This, however, gives to the criterion a negative form. If we seek to translate it into something positive we can do so, under safeguards, in reference to heredity. An instance of instinctive behaviour is one the 'form' of which is inherited and not acquired by the animal that so behaves. If then a complex act is simply observed we may be quite unable to say whether, taken by itself, it is an instance of instinctive behaviour or not, but taken in the light of its history in the individual life the case is different. One sees for example a boy cycling and a duckling swimming. Each is an instance of behaviour; but as a matter of historical development, interpreted on the basis of observation, the boy has to learn to ride, whereas the duckling has not to learn to swim. The 'form' of cycling in boys is acquired; that of swimming in water-birds is inherited. In the duckling there is inherited ability to swim; in the boy the ability to cycle is acquired. In the duckling the enjoyment which accompanies the behaviour is as new in form to the bird as is the behaviour; in the boy the enjoyment in cycling grows and develops as the art of riding is gradually acquired. No doubt the conclusions we draw with regard to enjoyment or conscious accompaniment are inferential. The doctrine of correlation which I accept seeks to afford a reasoned basis for such inferences.

There are other features on which the biologist may lay stress for his special purpose; for example, that under like circumstances closely similar instances of instinctive behaviour are observable in all the members of the same more or less restricted sub-class of animals, *e.g.* males of the same species. This is a matter of statistical evidence. If this clause of a definition be accepted an instance of instinctive behaviour is the functional analogue of an instance of inherited structure. The plumage of a male bird has certain inherited characters; the bird's instinctive behaviour in 'display' has in like manner an inherited and unlearnt character.

(2) While the specific form of instinctive behaviour is the observable expression of unlearnt ability so to behave, instinctive enjoyment is its psychical correlate. This enjoyment is just the inner aspect of that reality (the psycho-organism) of which the observed behaviour is the outer aspect. Under distinguishing analysis the one has its ground in the inherited constitution of the organism, the other has its ground in the correlated psychical system. In the one aspect, as in the other aspect, there is, in some sense, retention from one generation to the

next. Such retention, which is sometimes called 'mnemic,' must here be taken for granted. Just now what calls for emphasis is that what is retained is not biologically a specific mode of behaviour nor psychologically this or that kind of enjoyment. At present we will fix our attention on the biological aspect of behaviour, on the understanding that the psychological aspect of enjoyment is correlated with it. Now it is pretty clear that the nest-building behaviour of the parent is not retained, as such, in the fertilised ova which give rise to the next generation of nest-builders. Nor is the plumage of the male Peacock Pheasant retained in the egg from which he is developed. What then is retained in that germinal substance which, for biological treatment, affords the continuous thread upon which the beads of successive generations of adults are strung? Now here we must remember that, for such treatment in accordance with the doctrine of germinal continuity, the beads are developed from the thread, but the thread is not developed from the beads. In other words, germ-cells are the parents of the body-cells which are concerned in plumage and in behaviour; but these body-cells are not the parents of germ-cells. The parents of germ-cells are other germ-cells in the thread of living substance. What then is retained in the germinal thread from which the beads of adult organisms are developed? Clearly neither plumage nor behaviour for they are functions of specialised groups of body-cells. What then? Can we say more than that some essential feature of the intrinsic constitution of the germinal substance from which the organism is developed persists? That intrinsic constitution is the ground of all changes which occur under appropriate extrinsic conditions. But since this is so general and comprehensive as to be of little use for the interpretation of particular instances we define it *ad hoc* in terms of some specific item of behaviour. We say that what is retained is not this or that mode of behaviour itself, but the ability so to behave. None the less, we speak elliptically of inherited behaviour (or enjoyment); and people know what we mean. On this understanding it is a plain matter of statistical evidence whether behaviour is inherited or not, and if so in what measure.

It is probably quite unnecessary to insist that heredity does not in any true sense make offspring resemble their parents. The fact, or rather the general truth, is that they *do* resemble them in a measure that can be evaluated. This measure may be expressed in the form of a 'coefficient.' Thus perfect resemblance of offspring and parents in any given respect would have 1 as its coefficient of heredity; no resemblance (if it occurred) would be 0. Actually observed resemblance would be somewhere between these values—say ·75. Now I am not acquainted with definite statistics with regard to instinctive behaviour. But in such cases of what I shall call low-level instinct, as swimming, diving, or flying in birds, the procedure of offspring, near the outset of life, very closely resembles that which their parents exhibited at the same stage of development; and the coefficient would, I suppose, be a near approximation to 1. Still, there would be some variation in the behaviour of the offspring of the same parents, on either side of the most frequent measure or the 'mode,' in the technical sense, which, by a play upon the word, we may regard as the mode of procedure normal to the species. According to the theory of natural selection, those whose behaviour falls seriously below the normal mode of procedure are eliminated. Hence under such elimination the coefficient of hereditary transmission retains a value more or less closely approximating to 1, say, at a guess, ·95, with some slight range of variation above and below this value.

What we have been dealing with is such ability as is, on the evidence, unlearnt; and there are numberless instances of instinctive behaviour concerning which the evidence is quite convincing to an unprejudiced observer. Of these, the swimming of a duckling, a few hours old, is one. But there is also acquired ability of which we have taken a boy's cycling as an instance. The question therefore arises: Does not that also depend on heredity, as the phrase goes? It does, in so far as there is inherited ability to learn to ride. Here, however, the word 'ability' is used to cover both performing and learning to perform. Since this may lead to ambiguity, and since we have two words—'ability' and 'capacity'—may it not be well to differentiate? Let us then speak of *ability* to behave (*a*) without learning, and (*b*) through learning; and *capacity* to learn or acquire. The boy's capacity to learn to ride a bicycle is certainly inherited; otherwise he could never acquire the art of cycling. Wherein, then, does the duckling that swims differ from the boy that cycles? The former has inherited ability; the latter has inherited capacity, but the ability must be acquired. This differentiation of meaning is not, I hope, unduly strained. It is what we have in mind when we say that some pupil has plenty of capacity for, say, mathematics, though he has, from lack of training or want of application, little ability to solve problems. It is convenient to speak of inherited capacity as *innate*, and to reserve the word 'instinctive' for inherited ability to behave in some specific manner without learning. On these terms, intelligence as the capacity of learning through experience is innate; the intelligent *act* which depends on such learning is acquired by the individual; and behaviour thus acquired is to be distinguished from instinctive behaviour a criterion of which is that it is not so acquired.

(3) Thus far I have sought to distinguish (1) instinctive behaviour as unlearnt from that which we may now call intelligent behaviour the form of which is the outcome of learning; and (2) inherited ability to behave in some definite way without having to learn how so to act, from innate capacity to acquire some modification, let us say improvement, in the form of behaviour.

We may here pause to note that by intelligent behaviour we are to understand that which is learnt through profiting by experience. It is the outcome of guidance and control and in that sense is acquired in the course of individual life. Physiologically regarded, it involves a loop-line neuronic path, and the playing down of controlling centres on those which lie below them in the hierarchy of neural arcs. On these terms instinctive behaviour is logically distinguishable from intelligent behaviour; for the former is unlearnt and the latter is the outcome of learning. But it may be said, by Prof. Stout for example[1], that the acquisition of psychical data is part of the process of learning, and is therefore included in the intelligence which learning implies. There is, however, a valid distinction between the *provision* of data and the *acquisition* of data, if this be held to imply some seeking and choosing. The latter involves intelligent process; the former does not. My position is that the provision and psychical registration of data through unlearnt behaviour does not itself imply intelligent process. There may no doubt be, and in many cases is, innate capacity for dealing with such data; but we should distinguish between the intelligent process of so dealing with them, the outcome of which is intelligent behaviour, and the initial provision of data through instinctive behaviour.

The next thing clearly to realise is that the instances of behaviour which we commonly find in animal life are partly unlearnt and partly learnt. In other words there is an instinctive component or factor and an intelligent factor in the blended product which is what, on most occasions, we actually observe. And it is the difficult task of analysis to distinguish these two factors which combine in inseparable union to give the behaviour we see. Take for example the flight of an adult swallow. The young bird flies passably well the very first time it is committed to the wing. It has unlearnt ability so to fly. But it learns to fly far better. To its unlearnt ability there is added much improvement through acquired excellence, in virtue of the bird's innate capacity for profiting by experience. What therefore we commonly call 'an instinct' (which means an instance of instinctive behaviour) in adult life is probably never an instance of what may be spoken of as 'pure instinct.' It is an instance of behaviour in which the instinctive factor is still in evidence. Notwithstanding much acquired grace and ease, the swallow's flight is still traceable back to a stage at which the form of behaviour was unlearnt.

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[1] *Manual of Psychology*, 1913, 350.

Now the task of analysis is admittedly difficult; more difficult in some cases than in others; most difficult of all in that of human behaviour. This is partly because so much of that behaviour, even in the infant, is not traceable back to definite unlearnt forms, but to what seems to be a relatively formless assemblage of random and, in themselves, aimless movements. This affords much of the matter to which form is given in the course of personal development. There is probably less unlearnt ability in man than in any other organism, as there is more innate capacity for learning new forms of behaviour. But there is some; and the aim of scientific research is to discover it. There are, therefore, quite reasonable grounds for saying that the distinction between unlearnt (racial) and individually acquired forms of behaviour is sometimes so difficult to apply that it has then but little practical value[1]. But that does not invalidate the logical distinction; nor does it preclude our seeking instances in which it is susceptible of practical application. Such instances abound in animal life. There we often find serviceable forms of instinctive behaviour which, so far as the evidence afforded by careful observation goes, are unlearnt and common to all like members of the same species.

We have, then, to follow up their history in the individual life. We must ask: Does the performance show signs of improvement under intelligent guidance? Is there something more than smoother running as the organic 'mechanism' 'finds itself'? It is a matter of careful observation supplemented by cautious inference. In some cases there are unmistakable signs of improvement; in other cases there are not. The latter clearly approximate more closely to instances of 'purely' instinctive behaviour than the former. There is little sign of improvement, under intelligent guidance, in many of the instincts of insects. The nest-building of some birds seems often as true to type on the first as on later occasions. But very much of the locomotor behaviour of vertebrates—running, swimming, diving, flying—shows marked signs of improvement. The inherited ability to behave passably well is supplemented by much acquired skill.

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[1] Cf. Rivers, *Instinct and the Unconscious*, 41; cf. this *Journal*, x. 1 ff.

There need not be any knowledge, as we count knowledge, of how the betterment comes in the course of experience. It just comes, here a little and there a little; and it carries the naïve satisfaction which gives savour to active life. Which of us, with our human prejudice in favour of intellectual knowledge, knows much more than that, by some process of selection, unconscious or subconscious, redundancies and awkwardness which at first led to comparative failure are gradually eliminated and that just the essential business part of the skilled behaviour is knit into a nicely co-ordinated whole? Then it looks so easy in another, and in oneself the enjoyment runs so glibly. The youngster who rides his bicycle with nonchalent ease has very little detailed knowledge of how it is done. Ask him which way he turns his handle-bar when he heels over a little to the left; he is puzzled and often gives the wrong answer. But he *does* it, on the cycle, right enough. The swallow with yet more consummate mastery of a yet more difficult art probably has no knowledge whatever (of the intellectual kind) about the hows and whys of aerial flight. His not the why to give: his but to do and live.

(4) Instinctive behaviour is just an integral phase in the current life of the organism. We may distinguish its characteristic features; but we cannot profitably deal with it apart from its context; for it is what it is in relation to that context. Furthermore, to behave in this way or that, under given circumstances, reveals the nature of the animal which so acts. Thus we have, as noted above, (1) the behaviour itself, (2) its ground in the constitution of the organism, (3) the external conditions under which it is evoked, and (4) the outcome which we, on the basis of more extended study, regard as having prospective value or utility. The aim of analysis must not be misunderstood. It is not to separate and assign independent existence to nature's inseparables; it is to distinguish whatsoever may be distinguishable (and helpful to distinguish) within an integrated whole. Even so, however, something more must be added. For even in comparatively simple instances there is a current *process* of behaving within which there are sequent phases. And if we, so to speak, take a snap-shot of it in the midst of its course, the phase we thus isolate for consideration is what it is partly in virtue of precedent phases in a continuously developing process. We must realise, therefore, that behaviour, under (1) above, is an integrated and fluent whole which is susceptible of further analysis into the sequent

phases of its development. When we further bear in mind that under 'instinctive' we are to deal with the unlearnt *factor*, without prejudice to acquired factors which may be, and perhaps in some measure nearly always are, compresent therewith, we are in a position to consider that instinctive enjoyment, or consciousness in the broad sense, which is correlated with instinctive behaviour.

Now if we are to take correlation seriously, and if the foregoing treatment of instinctive behaviour be provisionally accepted and not unwarrantably confused with other applications of the adjective 'instinctive' (*e.g.* to knowledge or to impulse), then the psychical analysis must proceed *pari passu* with that applied to behaviour as objectively studied by the biologist. On these terms instinctive enjoyment is that which is the psychical correlate or accompaniment of the instinctive behaviour itself under (1). It is grounded in the constitution of the psychical system (2); it is evoked by the external conditions of sensory presentation (3); it has prospective value of the psychical order in that it provides data for subsequent use in what we speak of as intelligent profiting by experience (4). But what it *is*, as instinctive enjoyment, is, not what it is grounded in, not what it is evoked by, not its utility in mental development—important as all these are—, but just what it actually *is*, namely, the psychical correlate of instinctive *behaviour*. That it is, and nothing else than that, for my treatment under the method of distinguishing analysis. If this be not grasped, I cherish no hope that the thesis I seek here to develop will be understood.

I shall on another occasion say what I can in support of a suggestion that the word 'consciousness' may be used in a broader and in a narrower sense. In the broader sense it comprises all those instances of being conscious which are said to be 'above the threshold' (however that may be defined). In a more restricted sense it comprises only those instances which carry what James would call a 'fringe' of revival or expectancy. The criteria of being conscious in this narrower sense are (*a*) an enjoyment of againness when *this* which is now presented resembles *that* which was experienced on former occasions, and (*b*) a feeling of expectancy that something is coming like that which came on former occasions. These criteria are, as I hold, distinctively psychical and are what I call 'qualia' of conscious enjoyment, the validity of which Prof. Alexander so strenuously denies. Now that which comes with the quale of againness is that the like of which has already been given in the course of previous psychical life in the individual. And that which comes with the quale of expectancy in enjoyment is that the like of which has already come in past experience. But neither the one nor the other can qualify the enjoyment that accompanies instinctive behaviour as such, *i.e.* the instinctive factor in behaviour which is distinguishable under analysis. For when it first comes it is in form as new in the psychical life of the individual as is the behaviour in the organic life of that animal. The instinctive enjoyment which is correlated with instinctive behaviour is accompaniment and accompaniment only (epiphenomenal if you like the word), without guiding efficacy though it provides the data for conscious guidance in the life of fuller and further development.

The stress then here, for resolutely analytic treatment, is on the locus of origin of integration. Instinctive behaviour, with its correlated enjoyment, is an integrated product. But this integration is not (as it is in the intelligent factors in behaviour) of conscious origin, in the restricted sense of the word 'conscious.' It is integration in that part of the psychical system which is analytically distinguishable as (in this sense) the unconscious. But in the broader sense of the word 'consciousness,' which includes all modes of felt enjoyment, it may be thrillingly conscious. What then distinguishes the *restrictedly* conscious factor in the total enjoyment? Its newness, its freshness; the absence of againness or expectancy. When the duckling first swims—when the moorhen first dives—a quite new 'form' of experience (even if the 'matter' be old) breaks in on the already current stream of enjoyment. There is no feeling of againness; for there is no againness in the individual experience to feel. There is no expectancy of what will be the outcome of so behaving; for within that experience such outcome has never been presented. Only on the hypothesis that what is learnt in one generation is remembered by the next, can there be any feeling of againness or of expectancy. On that hypothesis (in support of which I can find no satisfactory evidence)[1] the duckling would have a feeling of againness and expectancy based upon so-called ancestral memory. It would remember—not indeed its own previous mode of behaving and its outcome, for it has never so behaved—but that of its parents and grand-parents.

(5) We live in an age which is somewhat impatient of analysis. The cry is: Let us deal with concrete reality in all its varied richness, not with bloodless logical abstractions. But does the analyst here disagree? Surely not, if he hold firmly to the sound view that the necessary complement of analysis is synthesis. We start with some complex whole which is the reality presented for our interpretation; by analysis, so far

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[1] Cf. Stout, *Manual*, p. 496.

as is possible, we evaluate the factors; then follows synthesis by which an imaginative thought-construct is framed which, if it may be, shall correspond to the reality with which we started, and shall enable us to grasp the nature of that reality more adequately than we did to begin with.

Now, suppose we start with a given instance of behaviour. We want to know all we can about it. We find that as a matter of history it is partly unlearnt and partly learnt. We consider it as evoked under certain external conditions; as leading somewhither in its outcome; as revealing the nature of the organism that behaves; as correlated with enjoyment which contributes in some measure to the manner in which the animal is conscious (in the broad sense) at the time. How can we distinguish these several items within a complex whole save by analysis? How can we grasp the true nature of what actually takes form in real life save through intellectual synthesis or ideal construction?

There is trouble, however, when we have to employ technical names. If we use the adjective 'instinctive' to qualify that which we name 'behaviour' in so far as it is unlearnt, we apply it analytically to that which we distinguish within the complex reality. But if we apply it to the synthetic complex we inevitably use it with a different connotation. For thus synthetically used, it covers much more than it does when it is used analytically. I venture to urge that its value as a qualifying adjective lies in its analytic usage.

But why select this one when others are open to our choice? Of any technical name the connotation (as I use the word)[1] is a matter of convention. The pity of it is that there is so little agreement in this case. We shall have therefore to consider some other applications of the word 'instinctive,' and just now its application to knowledge. We here include in our purview that which evokes behaviour. The kind of knowledge in question is that of immediate sensory acquaintance—that which is not mediately learnt. This it is that is said to warrant the application of the limiting word 'instinctive.' Now, suppose that it be asserted that an animal has instinctive knowledge of *this* 'as good for food,' of *that* 'as distasteful,' or of some other animal 'as a natural enemy.' For this, by the way, apart from any acquired knowledge, there is, I believe, very little evidence; but let that pass, important as it is. Put the thing very generally, as it is often put. The animal has, it is said, 'instinctive knowledge' how to act under these or those circumstances. What is the evidence? We observe that under such conditions the animal does so act. And then we say: How could it possibly so act unless it knew how to act? But I submit that this assumed knowledge is not in the evidence. Here, however, much turns on what we are to understand by knowledge. If it be said that the enjoyment involved in any form of sensory presentation is a kind of knowledge, *cadit quæstio*. For obviously enjoyment in behaving is such a kind of knowledge. But surely this is not what is meant when the phrase 'instinctive knowledge' is used. To know how to act without any prior experience of so acting is if I mistake not something quite distinguishable from enjoying the action in progress. And I contend that *this* is neither given in the evidence nor necessary to the interpretation of that which is so given.

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[1] Cf. Keynes, *Formal Logic*, Part I, Ch. II, § 17 (1).

The assumption of inherited knowledge (not to be confused of course with hereditary capacity for learning to know) may, however, take a different form—that of instinctive prevision—expectancy of what is likely to come although the like has never been experienced. This is clearly connected with what has been called above the outcome, or utility, or prospective value. In this form, what is given at the outset is not only how to act under certain given circumstances but, however dimly foreseen, the purpose or end of the act. Prof. Stout accepts such instinctive prevision as always present, though often in a guise that is inexpressibly vague, but yet not wholly indeterminate[1]. "Animals," he says, "in their instinctive actions do actually behave, from the outset, as if they were continuously interested in the development of what is for them one and the same situation or course of events; they actually behave as if they were continuously attentive, looking forward beyond the immediately present experience in preparation for what is to come[2]." I suppose no one dreams of denying these 'as if's.' The bird builds a nest as if it foresaw the laying of eggs therein. But does it foresee? I confess that I place no confidence whatever in the whole brood of 'as if's.' I must continue therefore to lay stress on instinctive behaviour, leaving instinctive knowledge and prevision to M. Bergson on the one hand and Prof. Stout on the other.

(6) Let us now consider the relation of intelligence to what I call low-level cases of instinctive behaviour, and try to correlate this psychical aspect with the physiological concomitants. It will involve some rather technical treatment. We must bear in mind that the function of intelligence is to control and to guide; and that physiologically this always involves neuronic loop-lines.

[1] *Manual*, p. 355.

[2] *Loc. cit.* p. 351.

In the accompanying diagram, which is drastically schematic, *D* stands for the receptors of all kinds, including those which receive kinaesthetic stimulation from the organs concerned in behaviour *C*. Under *B* fall the subcortical centres which are concerned in purely instinctive behaviour of low-level kind. *A* is the cortex, with the controlling function of which intelligent consciousness is correlated. Its shaded part is correlated with reflective thought where that is in evidence, *e.g.* in man.

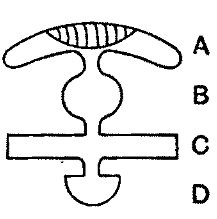

Fig. 1.

A Cortex that controls.
B Subcortical centres.
C Organs of behaviour.
D Sensory and kinaesthetic receptors.

Now, suppose that the connexion between *A* and *B* is severed. In that case we should have instinctive behaviour not merely logically distinguished, but physiologically isolated[1]. What then happens? Something of this sort. The sensory receptors in *D* are stimulated by the external situation; afferent physiological impulses proceed to *B*; and thence a co-ordinated set of efferent impulses pass to *C* and there is some definite form of behaviour. But thereby kinaesthetic receptors in *D* are stimulated; impulses proceed to *B*, and are reflected back on to *C*, modifying further behaviour. The behaviour, however, leads to fresh developments in the situation. Often other sensory receptors are stimulated, as is the case when a chick, in pecking, takes something into the mouth. *B* is further influenced and a new form of behaviour (*e.g.* swallowing or rejecting the food) results; and this new form of behaviour, and its outcome, yet further influences *B*. There is thus a series of stimulations and a chain of behaviour as, in and through the behaviour, the situation develops. The integrative action of *B* at any given moment is in accordance with the whole pattern of a constellation or configuration of neuronic influences. What happens is of the same kind as that which Prof. Sherrington has so brilliantly demonstrated in the 'spinal animal,' especially in its relation to what may be called the 'this or that' principle —where *this* and *that* are incompatible responses. One does not get a nondescript muddle-up of one and the other. This is of quite fundamental importance, and without prejudice to the 'all or none' principle[2] which has also to be reckoned with. It is not yet adequately realised to how large an extent the physiological conditions of 'conflict,' and of the so-called 'barrier,' are laid in the 'this or that' principle. In instinctive procedure this is endorsed by natural selection, as is, on occasion, the 'all or none' reaction. It is tolerably obvious that the highly specialised behaviour-attitude of immobility—so different in little plover and in pheasantling—is biologically incompatible with either fight or flight at the same time, though *this*, *that*, and *the other*, may afford examples of 'alternation'; and that to be of protective value there must be 'all or none' of such immobility, though fight or flight may be, and observably are in normal life, graded according to circumstances.

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[1] Cf. *Instinct and Experience*, Ch. III.

[2] See W. H. R. Rivers, *Instinct and the Unconscious* and cf. C. S. Myers, this *Journal*, 1913, VI. 141 ff.

Even low-level forms of instinctive behaviour reach in themselves a relatively high level of complexity. Furthermore—to add complication to that which is already sufficiently complex—the physiological impulses reflected down from *B* influence not only the organs of behaviour, but the whole organism, altering its physiological balance and, in correlation therewith, the emotional tone of enjoyment. This we must leave on one side, though we must bear in mind that how the organism actually behaves depends in large measure on the existing poise of the whole vital system.

Essential features, then, of low-level instinctive behaviour are (1) that it depends on integrative action in *B*; (2) that it carries into a higher phase of life such physiological principles as 'all or none' and 'this or that'; and (3) that it is, within some range of variation, equal to the biological occasion or has survival value.

Thus far we have supposed the connexion between *A* and *B* to have been severed. We are now to regard that connexion as remaining intact. What then happens? Something of this sort, stated as briefly and in as simplified form as possible. When the sensory receptors in *D* are stimulated under the presentation of the situation, afferent impulses proceed to *B*, are reflected as before on to *C*, but *also* pass up from *B* to *A*. Similarly kinaesthetic receptors send impulses to *B*, which are in like manner handed on to *A*. In general *all* that is sent up to *B* may be transmitted to *A*. From the inherited build of the nervous system, *A* may (to speak picturesquely) receive information through *B* of that which occurs in the body as the result of its integrative intervention. And the information with regard to behaving is the instinctive enjoyment which accompanies this low-level instinctive behaviour in so far as it provides data for control under the functional action of *A*. Under this proviso it is the psychical correlate of the functioning of certain centres in the cortex of the brain or its analogue (*e.g.* in birds).

It must not be supposed that enjoyment is necessarily limited to correlation with the functioning of the cortex or its analogue. More probably it is the psychical correlate of the functioning of *the whole organism*. Consciousness is based on and in touch with the unconscious, which is psychical in its nature, and not with physiological processes as such—*i.e.* only with their inner aspect in enjoyment. One does not deny—nay rather one gladly accepts—'protopathic' enjoyment in the thalamus and perhaps elsewhere. The point is that only when the enjoyment is also 'epicritic' does it enter into the field of discriminative control which characterizes the integrative business of *A*. It is this utilisable information which counts in psychological development.

But the reception of information is one thing; its utilisation in control is another. There must first be information of instinctive behaviour in progress; then follows control in the light of this information. And this control is the mark of intelligent process—not the receipt of information, but its utilisation in action through the playing down of *A* on the underlying centres in *B* or on centres in the spinal cord.

There seem then to have been evolved in close inter-connexion two systems—that of *B* and that of *A*. The functional activity of *A* depends on that of *B*; but initially the functional activity of *B* does not depend on that of *A*. Each is integrative in its action. But the secondary integration due to *A* may modify the primary integration due to *B*. That is what is meant by saying that instinct may be 'plastic' and may be moulded to finer issues. Then, however, it ceases to be purely instinctive.

Only in certain organisms, however, and in certain fields of their behaviour, is there *B*-integration *in instinctive form*. In some animals much of the initial behaviour-stuff consists of relatively random movements comparatively destitute of integrated form as a whole. In such cases, though random movements supply the 'matter,' the form of behaviour as a whole is acquired and due to intelligent guidance under the influence of *A*. This is markedly so in the higher animals, with protected infancy, where parents shield the offspring from the eliminating effects of natural selection. It is conspicuously so in man. There is, therefore, a central core of truth in the old-fashioned antithesis between the instincts of the lower animals and the intelligence of man. No doubt in one large field of behaviour—that connected with reproduction and the relation of the sexes which is subsequent to the period of infancy—there is in man a recognisable legacy of instinctive form, as Freud and his disciples insist. But marking as it does the period of adolescence—coming as it does after so much has been told and read about it, and after manifold warnings of the danger to the moral life which besets its advent—how difficult is the task of distinguishing what are here regarded as the purely instinctive factors! But when all is said and done, we must realise that the human subject is the very last which the investigator of instinctive behaviour should select as a basis for interpretation, or for purposes of illustration.

A word or two of warning may here be necessary in view of a different application of the word 'instinctive.' It is quite commonly used in contradistinction to 'reflective.' If this or that mode of procedure of men in social life has not come to be what it is under the guidance of reflective thought, it is said to be instinctive. This does not mean that the procedure in question is unlearnt; for it is in large measure learnt from cradle-days onwards. It does not even seek to emphasize the presence of such an unlearnt factor as that with which we have been dealing. What it seeks to emphasize is the absence of reflective thought—the absence of fully rational guidance. For such 'instinctive' procedure (in this sense) we often trump up reasons by a process which is nowadays called 'rationalisation.' Perhaps nine times out of ten something of this sort is the meaning of the word 'instinctive' in popular speech. It is, of course, quite a legitimate usage under suitable definition. If I mistake not, Prof. Alexander always uses it thus. It is the usage which Mr Trotter's treatment of the 'herd-instinct,' and Freud's comprehensive treatment of the sexual instinct, have done much to render current. It would be foolish to raise any objection to this usage. Still it should be sufficiently clear that much behaviour that is in large measure the outcome of much personal experience is *in this sense* instinctive, but at the same time not wholly, or even mainly, instinctive in the narrower sense we have here in view. To confuse the one usage with the other can only lead to intellectual smudginess.

To summarise the position, there are three main levels of integration: (1) reflective integration in the sphere of thought-processes; (2) integration in the sphere of naïvely perceptual and unreflective profiting by experience; and (3) integration of the unlearnt order. As I use the word 'instinctive,' so far, the behaviour thus distinguished falls under (3). But the word is often applied to both (2) and (3).

(7) Leaving reflective thought (which is the prerogative of man) on one side, I distinguished above instinctive *B*-integration from intelligent *A*-integration, the former primary, the latter secondary. In some animals, and in some fields of behaviour, the former predominates; in other animals, or in other fields, the latter. The flight of an adult swallow is

a blended product. It retains some measure of the primary integration (in evidence on the first occasion of flight); but this has been modified and much improved by the secondary integration through which it is controlled and rendered more serviceable in the life of the bird. A much debated question here arises: Is the acquired excellence due to *A*-integration in one generation, inherited as *B*-integration in the next? Some say: Yes. Others say: No. It is a matter of statistical evidence into which we cannot here enter. Let us then put it in hypothetical form. If the Noes have it, what then? Then, from the racial point of view, instinctive behaviour is not the offspring of intelligence, though intelligence may play the part of kindly aunt who ministers to its survival in the species. It should be noted that this does not imply that the evolution of instinctive behaviour is necessarily independent of intelligence; for it may be largely dependent on the good offices of the aunt. It does, however, imply that the aunt who sustains it and nurses it, who teaches it and guides its steps, who makes it through training other than it was, is not its mother. Apart from its instinctive mother it would not be born; apart from the aunt it might not be reared; it might perish and have no heirs. But what the behaviour actually becomes in later life is partly derived by direct inheritance from the instinctive mother and partly due to the teaching of the intelligent aunt who dwells in the same household of the organism.

One has to distinguish in what I have called the *B*-system *variations* in nature, around the specific 'mode,' of germinal origin, and *modifications* acquired under the nurture provided by *A*. The survival of the organism depends on both when both are compresent. Let us then tabulate the possible combinations.

Variations and modifications in B-system.

(1) Favourable variation *with* favourable modification due to *A*.
(2) Favourable variation *without* favourable modification due to *A*.
(3) Unfavourable variation *with* favourable modification due to *A*.
(4) Unfavourable variation *without* favourable modification due to *A*.

In this table unfavourable *modifications* are not entered because animals in their free life do not contract many bad habits.

In the first class are those which are strong by nature and stronger still through nurture; in the fourth class are those which are weak by nature and have not been improved through nurture. In the first class are those that inherit good instinctive ability which is supplemented by acquired ability; in the fourth class are those with poor natural ability which remains no better than it was at first. It seems pretty evident that in the struggle for life an animal in the first class will stand the best chance of survival and of propagating his like; while one in the fourth class will run the greatest risk of being eliminated, without begetting heirs to his weakness. The latter will be the first to go; the former will be the last to stay. If that be so, the best all-round animals will survive, not only because they are first-class by instinctive nature, but also because they are first-class too through intelligent nurture. Acquired ability will therefore favour the survival of those who possess unlearnt ability *along the same line of performance*. And, in the absence of very careful analysis, it will look 'as if' acquired ability were inherited as such; which it may not be, and under our hypothesis is not[1].

Of course in concrete fact there is further complication in that the *A*-system has its innate capacity for guidance, and its hereditary modes of controlling behaviour; while, at any rate in man, naïvely perceptual intelligence comes under the yet higher control of reflective thought. Of all this we need here say nothing more.

It must suffice to add that, if we entertain the hypothesis that in some animals an *A*-system is differentiated from a *B*-system, this may mean, as M. Bergson puts it, that neither is intelligence directly evolved from instinct, nor is instinct directly evolved from intelligence, though they may co-exist side by side in the same animal. Instinct and intelligence are on different lines of evolution. But both lines may contribute to the evolution of the same animal, and may co-operate in the furtherance of its life as a whole.

(8) Before passing on, we have next to notice that there are levels of ascending complexity in what we may broadly speak of as instinctive behaviour. At a relatively low level[2] stand such primitive instincts as swimming, diving, flying, showing fight, crouching when startled, and so forth. An example of instinctive behaviour at a much higher level may be given in some little detail. One of the most interesting instincts of bird life is that which Mr Eliot Howard speaks of as 'securing a territory.' Among migrant birds the male, often one in his first year which has never done the like before, arrives here in England a week or two in advance of the females, and forthwith proceeds to 'stake out his claim.' Let us, however, hear Mr Howard himself. If we "watch the

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[1] Cf. *Habit and Instinct*, Ch. xiv.

[2] I insert here a passage from an article in *Scientia*, xxviii. 272. I there suggest a provisional scheme of progressive integration.

male of one of our common species—a Reed Bunting for instance—and record its movements each day during the first hours of daylight from about the second week in February, what do we find? Not that its behaviour is of a casual description, not that it is here to-day and gone to-morrow, but that all its movements are subject to a routine which becomes increasingly definite as the season advances. There it is in the same plot of ground, in the same bush or clump of bushes, taking short flights in one direction and then in another, attacking other males that come within a certain radius, and exhibiting in all its actions a strong disposition to make its own just that one particular corner of the universe. Those few acres wherein it performs this routine of activities and awaits a female I have termed a breeding territory, the dimensions of which vary according to the species and according to the environment[1]." Now all this has for the naturalist prospective value. It is the normal precursor of mating and all that follows thereon. But the point which I wish here to emphasize is that there is probably not a single one of the many low-level instincts incorporated in the procedure as a whole, which has not undergone much modification—which has not been perfected through a lifetime's profiting by experience. Flying to and fro, perching on a chosen bush, attacking other males—one may say *all* the movements which are subject to that definite routine which Mr Howard so well describes in this and many other passages—are the perfected acts of a bird that has long ago learnt to use them in many and varied situations. In a sense there is nothing new; in another sense the procedure is conspicuously new. It is the 'form' that is new, while the 'matter,' which is incorporated in this form, is old.

This distinction between matter and form is, I suppose, sufficiently familiar. It is, of course, a distinction and not a separation. Following Aristotle, we may say that there is no matter without some form; nor can there be any form without some matter which takes on that relational form. Thus we may pay attention to the 'form' of a hexameter irrespective of the particular words which give the 'matter' of any line in the Homeric poems, in the *Æneid*, or in *Evangeline*. So too, we may grasp the form of a geometrical progression, though the numbers which have relation to each other under this common form are indefinitely variable. But in neither case can one have the form existing in complete independence of *any* matter. Furthermore, just as there may be in a hexameter new matter in a form that is already familiar, so there may be, and often is, the old matter of well-worn significant words set by a poet in a form which is quite new and which one greets with glad surprise.

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[1] *British Warblers*, Pt IX, 2.

The bearing of all this should be obvious without elaboration. In the high-level instinctive act of securing a territory—carried out by a male in his first year who has never before so acted—there is, in a sense, nothing new, while, in another sense, the procedure is, in him, conspicuously new. New too must be the specific character of the instinctive enjoyment. The matter is old; the form is new. All the constituent activities—flying hither and thither, perching, fighting and the rest—are well-nigh as old as the bird himself. But this routine, now for the first time in evidence—the relational form which all this old matter assumes—that is new. We have a typical instance of integration unconscious in origin (in the restricted sense of consciousness). Nor is it only at this higher level of bird-life that old matter takes on new form. The muscles employed in the duckling's swim have perhaps all been used in previous walking; those employed in the moorhen's dive, in the swimming which preceded it. None the less they are used with a difference; the instinctive *form* of swimming is other than that of walking. The instinctive *form* of diving is different from that of swimming. The whole set and poise of the body is different; the limb-movements are observably different; respiration is suspended, and so forth. And correlated with all this, the form of instinctive enjoyment must be different.

(9) Instinctive behaviour has been the pivot on which our whole discussion has turned. In the adult life of vertebrate animals, however, there is only an instinctive factor in behaviour; and we have sought to trace its relation to another co-operant factor in virtue of the presence of which, behaviour is, in part, of intelligent origin. But, as was said above, we have to consider behaviour as evoked under certain external conditions, as leading somewhither in its outcome, and as revealing the nature of the psycho-organism that behaves. It is to this last-named feature that we have now to direct our attention; and in doing so, we have to realise that it is *this*, or some feature therein, and not the behaviour itself, that is regarded by many writers on the subject as the pivotal concept. The stress is then not on behaving in some observable way, but on an instinctive disposition so to behave, and not only to behave in bodily fashion, but more besides. Thus Prof. McDougall defines an instinct as "an inherited or innate psycho-physical disposition which determines its possessor to perceive, and to pay attention to,

objects of a certain class, to experience an emotional excitement of a particular quality upon perceiving such an object, and to act in regard to it in a particular manner, or, at least, to experience an impulse to such action[1]." Here instinct, as disposition, *determines* perception, emotion, and overt behaviour. But in the treatment that follows, it is the behaviour which is specially regarded as instinctive and its name is hyphened with the concurrent emotional state, *e.g.* flight-fear, pugnacity-anger. So too, what Dr Drever understands by instinct is "determinate conscious impulse which is not determined by previous individual experience, but which, nevertheless, enters into and determines individual experience and attitude[2]." The point here to be noticed is the shifting of emphasis from behaviour to disposition or to impulse, the application of the word instinctive to some 'determining' feature in the nature of an independent Mind or Psyche which controls behaviour, and the retention, in some measure, of the distinguishing criterion of innateness as contrasted with acquiredness.

Now if, apart from the animistic hypothesis, this shifting of emphasis conduces to more adequate interpretation of all the facts, it would be foolish to reject it. And where the universe of discourse is mainly restricted to 'instinct in man' it may on these terms be convenient to accept it. But it forms no part of my present aim to argue in favour of one course or the other. From the standpoint of the study of life (and not only of human life), I regard observable behaviour as the best departure platform; but that this depends on, or is grounded in, the nature of the being that behaves, no one is likely to deny.

One has here to take over from common speech words which may be shaped to technical usage with some change of significance. We have, for example, this array of names—nature, temperament, disposition, inclination, and impulse. Let us pause to consider briefly what the plain man understands to be their meaning[3]. I suppose he will regard 'nature' as the most comprehensive and as including both the physical and the psychical constitution. If he has occasion to use the word 'temperament' he understands it to refer to a specific kind of nature—melancholy, sanguine, and so forth—mainly in its mental aspect, but dependent on something 'in the blood.' Without denying a physical aspect to the other names on our list he will take them primarily in their mental aspect. For him, I take it, the disposition at any given time is a *felt* disposition. And, I suppose, he might regard the sequence, given

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[1] *Social Psychology*, 29.

[2] This *Journal*, x. 29. Cf. his *Instinct in Man*.

[3] Modified from *Scientia*, *loc. cit.*

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above, as implying a progressive focusing with concentration of activity. And if he be asked to draw some distinction between a disposition to smoke, an inclination to do so, and an impulse to light a cigarette; or (to extend the series) between a choleric temperament, an irritable disposition, an inclination to swear, and an impulse to say 'damn' when he misses his stroke at golf, he will perhaps say that it is a progressive series, which culminates in the expletive which relieves the tension; that he hardly feels the temperament which is an abiding trait of his nature; that the disposition is a more clearly felt manifestation of that trait; that the inclination is yet more insistent; while the impulse is still more strongly felt so that it requires some self-restraint to resist it. And I think he might add that to act under impulse does not imply any intention so to act; nay rather that, whereas when we act with intention we always have some end in view, when we act under impulse we do so without thought of the consequences. Thus I interpret the attitude of the man in the street or in the smoking-room.

(10) Now the word 'instinctive,' though it is used in a general way to qualify 'nature,' has a more specific application to 'disposition.' The first thing to notice is that in the technical sense advocated by some leading writers, a disposition, as such, is not within the field of experience. Thus Prof. Stout says that "dispositions themselves fall outside of experience[1]." Prof. McDougall urges that, *qua* psychical, they belong to the structure of the mind while consciousness arises in the course of its functional activity[2]. And Dr Drever says that "dispositions as such, though determining experience, are never themselves experienced[3]." Here there is a pretty wide departure from popular usage; for on these terms one may not speak of a felt disposition. One may not say that a Reed Bunting has a disposition to secure a territory if one means that it feels an inclination to do so. One might, of course, differentiate between a disposition as an unconscious urge (hormé of some writers) and a conscious inclination. But I question whether this is practicable or desirable. As matters now stand the trouble, in dealing with problems of animal life, is that what is written concerning dispositions in the sense advocated by Prof. Stout and others is pretty sure to be read as implying some mode of conscious enjoyment. In what follows I shall depart from current technical usage and deal with dispositions on the

[1] *Groundwork of Psychology*, 60.

[2] *Psychology*, Home University Library, Ch. III.

[3] This *Journal*, x. 28.

understanding that some measure of enjoyed experiencing may be included, or at any rate is not excluded. And should I have occasion to use the word 'impulse'—which occurs so frequently in Mr Eliot Howard's work on *Territory in Bird Life*—I should mean, in accordance as I think with popular usage, the focusing of a preparatory disposition on some action at the moment of actual behaviour or just before that moment. One ought perhaps to add that in the usage I suggest it is not the nature, nor the disposition, nor the impulse that acts, but the animal or the psycho-organism. One must be careful not so to hypostatize a disposition as to regard it as having independent existence. It is not, in strictness, a disposition or an impulse that is stimulated and acts, but some being so disposed that responds to appropriate stimulation.

One may first clear the ground for further treatment by noting that we commonly describe a disposition in terms of the prospective value of the behaviour which is its normal expression. We speak of a disposition to secure a territory, to mate, to build a nest, and so on. It is as appropriate to describe an instinctive disposition in terms of its biological utility as it is to describe a reflective act in terms of the intention or end in view. When we speak of a disposition, then, we have a double reference in mind, (*a*) to its outcome in the behaviour itself or its results, and (*b*) to its specific character as the existing poise or attitude of the nature or constitution at the time being. One cannot deal satisfactorily with the facts apart from this double reference, for behaviour is the evidence of the disposition and the disposition is that which is regarded as the ground of the behaviour. But it is with the second reference (*b*), as a matter of emphasis, that we are here chiefly concerned.

Now we place a duckling in water and it swims; we frighten a moorhen and it dives; we put grains of rice before a chick and it pecks; we tweak the down on a pheasantling's neck and it scratches. In such instances of low-level instinctive behaviour there is little point or value in distinguishing the disposition from the nature of the bird. Each does what it does under the given circumstances "for 'tis its nature to." And if we call self-preservation or race-maintenance instinctive (which some would not), here also to speak of a disposition is of little service to further interpretation. It suffices to say that such is the nature of psycho-organisms. But when the nature or constitution of the animal is in some way thrown into a state of preparedness for the further development of some situation—when in the absence of such a state of preparedness as affording an internal factor (*e.g.* sex-hunger) the behaviour under environing circumstances is different—then it seems really helpful to speak of a disposition. It means a state of *ad hoc* attunement of the animal for appropriate behaviour at a coming stage of some normal routine. And I think that we may fairly assume that such attuned preparedness is accompanied by enjoyment and does not necessarily fall outside of consciousness in the broader sense of the word. And this psychical aspect of the disposition is correlated with a state of physiological preparedness in the organism. There is a psycho-physical disposition.

Take an example which is discussed with fuller detail in Mr Eliot Howard's *Territory in Bird Life*[1]. During the winter, before the mating season, Lapwings live together in flocks. The males are seemingly on the most friendly terms with each other. Just such minor squabbles occur as may give some savour to the life of enjoyment. The females are, for the males, just other birds in the flock and are not viewed, so to speak, in the light of a disposition to mate. Then in due season come the changes which constitute *ad hoc* physiological and psychical preparedness. Now one male and now another leaves the flock and occupies a more or less circumscribed area in the fields; and, once there, he is intolerant of other males, fighting with any intruder for all he is worth. With the physiological change (partly due to specific internal secretions contributed to the blood-stream) there is a complete psychical change in the dispositional set of the bird's nature. Substantially the same presentation afforded by another male is now linked with quite different modes of overt behaviour. No longer gregarious he is a solitary occupant of a restricted domain driving all other males away. Presently he is joined by a mate, and the normal routine of reproduction runs its course. But if a cold snap should come on, the separated birds may reunite in a flock, and the male is then no longer intolerant of other males. The physiological state probably reverts to its winter poise, and with this is correlated a reversion to the previous psychical disposition. But it seems that when a male bird (Lapwing or other) has secured a territory and is intolerant therein of all others save a mate, he sometimes returns for a while to the flock which occupies a neutral area. *There* he is no longer intolerant of others, but moves among them on quite friendly terms. Hence under different circumstances (1) in his territory, and (2) with the flock, his disposition is different, so that occupancy of a territory appears to be a determining condition of the behaviour observed therein. If this sort of thing occurred in so marked a form in human life it would perhaps be regarded as a case of 'dissociation,' or even described as an instance of 'double personality.' In any case just thus, as it seems, does the male bird in his first year behave though he has never so behaved before nor seen others so behave[1].

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[1] *E.g.* pp. 58–61, 98 and 102.

Now in dealing above with low-level instinct, I regarded the integration as a function of the system I distinguished as *B*. Is the higher-level integration with which we have more lately been concerned to be regarded as likewise a function of *B*? We do not know. But I think it more probable that it is the hereditary factor in the functioning of *A*[2].

(11) Assume that this is so. Does it not upset the whole treatment of (6)? A distinction was there drawn between *B*-integration, in virtue of which the behaviour was said to be so far instinctive, and *A*-integration, in virtue of which the behaviour was said to be intelligently modified. *B* gives the instinctive factor; *A* the intelligent factor. That, it may be said, is clear enough, whether we agree with it or not. But now it seems to be suggested that there is an instinctive factor in the *A*-business. Surely here there is flagrant inconsistency.

But what was our fundamental criterion of instinctive behaviour? That it is, on the evidence, in form unlearnt in the course of individual life—or at any rate that there is distinguishable, under analysis, such an unlearnt factor. In the case of what I called *low-level* instinct (class 5 of my *Scientia* paper) I regarded the unlearnt factor in integration as having its home in *B*. Is it really inconsistent to suggest that in certain forms of higher-level instinct, there may also be an unlearnt factor in virtue of hereditary modes of the functioning of *A*? May not intelligent integration have its unlearnt 'form' dependent on the inherited synaptic pattern in *A*? I have, however, myself felt the difficulty and have elsewhere spoken of "hereditary dispositions and innate tendencies" in connexion with integration as dependent on *A*. This has been criticized; and I feel the weight of the criticism. Here and now I seek so far as I can to come into line with the views and the nomenclature of others. It does seem to be inconvenient, to say the least of it, to be precluded from speaking of the securing of a territory by male birds as instinctive. So I broaden my use of the word so as to include such higher forms of unlearnt behaviour. What I do *not* include (though others are free to do so under suitable definition) is that component of form which is due to acquisition in the course of individual life.

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[1] I have used here an extract from a review of Mr Howard's work contributed to *Nature*, Jan. 6th, 1921.

[2] Cf. *Instinct and Experience*, Ch. IV.

Now in the behaviour of birds (and there are also plenty of instances in the life of mammals) one can, if one has one's nose really up against observable facts, distinguish the unlearnt part of the form from that which is learnt. And it is pretty definite form—it is, if one may so put it, specific like the form of a hexameter and not generic like the form of verse. It is much more difficult to distinguish definitely specific form of the unlearnt order in human life—a form which is observable in all members of the same more or less restricted group of men. And yet one can, in some measure, distinguish that which comes by nature from that which is acquired through nurture.

Let us take those two conflicting tendencies in human life, recognised for centuries and abundantly illustrated in literature, which Prof. McDougall names self-assertion and self-abasement. It is difficult to hit upon satisfactory designations of antithetical forms of conduct each so very wide in its range of occurrence. Let us briefly consider the part they play in human life, though it has its counterpart, on a much lower plane, in animal life. In the one there is some looking down to (not necessarily looking down *on*) that which is at a lower level on the same line of development; in the other some looking up to that which is above us. Can there be any doubt that both these tendencies are there in the inherited make-up of each one of us? Can there be any question that both are manifested in very varying circumstances? Of course, broadly speaking, we may say that the one appears under these circumstances and the other under those. But how wide is the range of difference in these circumstances! And though fundamentally based on unlearnt foundations and thereby conforming to our definition of instinctive, how largely they are shaped under the teaching of acquired experience! The boy among 'grown-ups' in the drawing-room is (sometimes) delightfully submissive; in the nursery, among 'the kids' he lords it over others. In the company of those who know far more about some subject than we do, we look up to them from our lower level; presently among those who regard us among those who are counted as authorities, we find ourselves laying down the law from our higher standpoint. Some of us are unduly submissive in presence of difficulties; others have extravagant confidence in their power to grapple with and master any difficulty under the sun. In almost everything that we do, now one, and now the other, gets expression. The matter is as varied as life; the form, though it too varies, has features due to nature and not to nurture.

The teacher who knows his business plays upon these complementary traits in the characters of his pupils. If both be present by nature in something like due balance, he seeks to bring it about that each finds its proper place and time in the life of the boy or girl. If, say in the matter of difficulties, there is, in a diffident child, an unduly submissive attitude in their presence, he gives well-graded tasks which can be progressively mastered with increasing confidence. But if the child is by nature too cocksure, the grading of tasks is different. Overweening confidence has to be suppressed; the boy has to learn that there are difficulties he cannot readily overcome. The teacher has to help him to realise this and perhaps rub it in.

It is tolerably obvious, then, that if we speak of these as modes of instinctive behaviour—if with Prof. McDougall we place them on the list of man's 'primary instincts'—we should clearly realise to how large an extent the purely instinctive factor is masked by manifold acquisitions in the life of human folk. We should realise too that we can hardly be said to be dealing with a particular mode of behaviour, or one that involves perceiving and paying attention to objects of a certain class, or one that is linked with a particular emotional affect, unless the word 'particular' and the phrase 'of a certain class' are used in so broad a sense as to lose their usual signification. Still there is present an unlearnt core at the heart of so much that is learnt.

In some cases in human life it is customary to use the word instinctive for what is in a marked degree individual and personal. It is for example in accordance with current and long-established literary usage to speak of the creative intuition of the born artist as instinctive. Of him we say: *Nascitur non fit.* By this we mean that what is most distinctive of his life-work is not only, and not chiefly, the result of laborious acquisition. It is in large measure of unconscious and unlearnt origin. It wells up in him, he knows not how and can only guess whence. How impatient he often is of any psychological interpretation in terms only of what is acquired! It is *not* acquired, he protests; it just comes with glad surprise even to the artist himself whose life is brimful of the gloriously unexpected. That is the joy of it. But (this he will admit) mastery over the *technique* of his art has to be learnt; manipulative ability must, in large measure, be acquired. It is utilised, however, in subservience to the dominant artistic 'instinct.' For the most consummate skill, however consciously and conscientiously won, never yet made an artist. If there be no touch of unconscious genius, of what avail all this mastery over detail? No doubt it may be said that the male bird which secures a territory and acts his part in the drama of reproduction, has *his* touch of unconscious genius, though he too has acquired the necessary mastery of detail in his skilled behaviour. But *it is the genius of the species rather than of a highly endowed individual.* It is perhaps more nearly analogous to the racial traits of the Japanese. And though a comparison of human life and bird-life, from which so many of the foregoing illustrations have been taken, may be instituted on some such lines as these, we must remember that man has above all the capacity of reflecting on the ends to which his inherited tendencies are insensibly leading him on. If the human artist lives along the lines of native endowment, as does also, with a difference, the Blackcap as a consummate master of song, he has also, as man, the beckoning vision of a promised land to which he has not attained—only the outskirts of which he will live to reach. If hereditary racial traits differentiate the Frenchman from the Japanese, each has also his ideal of national development in the future. It is in this prevision of the unattained end to which an innate urge is contributory, and in the rational control of the acts which are expressive thereof, that we find what is most distinctive of human life. Whether the male bird in securing a territory has aught of this anticipatory vision—whether his procedure is subservient to an ideal—is, to say the least of it, open to serious question.

(12) If I were asked: "What are you really driving at in this and so much else that you have written on instinct?" the reply would be: "My aim has been to find its place, as defined, in the evolutionary story of life and of consciousness." Further points may be developed in dialogue form. "Do you mean by this (*a*) the manner in which the mind acts as a directive agency in determining the course of instinctive behaviour (as mere matter of observed fact), or (*b*) the way in which the mind acts as an impulsive agency—as the driving power behind the observed behaviour?" "I mean neither the one nor the other." "But you *must* include both; otherwise you give only a description in terms of observed facts and of truths (so called) which are merely generalisations that formulate the plan of the facts; you leave both the facts and the plan—still more what makes the facts *go* in accordance with plan—utterly unexplained." "Yes, that is so. I base my procedure on science as I conceive it. Science does not pretend to explain anything in the sense in which you use the word." "That may be all very well in physical science which has no immediate concern with mind as the source of all true activity, directive and impulsive. It is an impossible position when one deals with instinct, which can nowise be explained on mechanistic principles." "May I suggest that we drop the use of the word 'mechan-

istic' since it betrays a complete misapprehension of the view which you criticize?" "I confess I do not know how else to characterize that view. In what way would you state it broadly yet succinctly?" "In terms of relatedness. Alike in a field of purely mechanical relatedness, in one of physico-chemical relatedness, or of organic relatedness, or of conscious relatedness (awareness)—*i.e.* in any field of relatedness—given the varying 'density' of that field, and the configuration of terms therein, something happens. Our business is to state clearly what happens, and what is the plan of that happening." "But you are forgetting the fundamental question, what makes it happen and guides the course of its happening?" "All that we can know or surmise about this is derivative from the *de facto* happening. In science (again as I conceive its aim) we deal with facts and seek to formulate their plan." "But is not instinct, as a mental force, a fact at least as worthy of consideration as any of the physical forces?" "Force in this sense is not a scientific concept. It has been almost universally rejected in physical science. I venture to anticipate its rejection in any philosophy which is founded on scientific method." "No doubt it may have been wisely rejected in physical science. Its methodological exclusion there is justifiable because there is no activity—no vital impulsion—which intuitive experience alone can reveal." "You are doubtless aware that philosophers of standing, who are not specially tainted with what you may call materialism, have roundly denied the validity of your interpretation of this intuition." "Has not Bergson conclusively shown that their attitude is untenable?" "That is a matter of opinion. But does M. Bergson regard *l'élan vital* as a concept of science? I had thought that his contention is that science is quite incapable of revealing this vital impulsion[1]." "Be that as it may, at all events modern psychology as a science is not so narrowly pedantic as to disregard such facts as these. And all progressive discussion of instinct defines it in terms of mental activity, impulse, and driving force."

"Do you speak of 'progressive discussion' in England, or in America, or elsewhere?" "No doubt there is in the United States a school of behaviourists, so called,—I half suspect you are an alien member of the school—whose aim apparently is to deal with psychology irrespective of consciousness." "What do you suppose they here mean by consciousness?" "That which plays the part of the Prince in the psychological play—the activity of mind as an agent." "I think you are right. They follow the lead of Wm James in his answer to the question: Does consciousness exist? Most of them say that it exists as a function but not as an agent; and not only they but, if I mistake not, the majority of American psychologists." "I really fail to see the bearing of all this. Are we not wandering from the main issue?" "And that is—?" "Well, I am still perplexed as to what may be your philosophical aim, if you have one." "Here as elsewhere, and perchance more fully in a forthcoming work, to revert to the teaching of Spinoza, and to restate it in modernised form. One corollary of his doctrine is that there is no interaction between instinctive behaviour, as a mode within the attribute of 'extension' and instinctive enjoyment, as a mode within the attribute of 'thought' or of consciousness." "Thus does he, and do you, leave the driving power behind what you are pleased to name instinct, and behind every act properly so called wholly inexplicable." "Had not Spinoza something to say about God?"

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[1] Cf. *Creative Evolution*, Eng. Trans. 171.

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IS A FATIGUE TEST POSSIBLE?

BY B. MUSCIO.

(*A Report to the Industrial Fatigue Research Board.*)

I. The conditions of search for a fatigue test (pp. 31–33).
II. The extent to which these conditions can be realised (pp. 33–41).
III. Proposed fatigue tests (pp. 41–44).
IV. Conclusions and suggestions (pp. 45–46).

I. THE CONDITIONS OF SEARCH FOR A FATIGUE TEST.

THE conditions of experimentation with the purpose of finding a fatigue test are two:

(*a*) that we know what we mean by fatigue;
(*b*) that we have some method *other than the use of a suggested fatigue test* by which we can *know* that different degrees of fatigue are present at certain different times.

(*a*) That the first of these conditions is necessary is self-evident: it is obviously absurd to set about finding a test of an undefined entity.

Strictly, however, it would be sufficient if we knew that certain different degrees of fatigue were present at certain different times (without knowing the nature of fatigue); but it is in fact impossible to know this so long as the nature of fatigue is unknown. Nevertheless, it is not necessary that our knowledge of the nature of fatigue be exhaustive; incomplete knowledge, of a particular kind, *about* fatigue would be sufficient to render the search for a fatigue test reasonable. This knowledge would need to be of the form: fatigue, though not known exhaustively, has a specified characteristic expression. It would be necessary to possess this as the minimum of knowledge before we could know in what degree, if any, fatigue were present at certain times.

(*b*) If it be not known *before the application of a proposed fatigue test* in what degree fatigue is present, the result obtained from its application will be destitute of significance. The results of any proposed fatigue test, in so far as it is a fatigue test, must correspond exactly with the presence of different degrees of fatigue; and the determination of such correspondence requires a knowledge of *two series*, namely, *on the one hand*, a series consisting of results obtained from the application of the test at different times, and *on the other hand*, a series consisting of the different degrees of fatigue present when these results were obtained. The knowledge of this second series must be obtained by some other means than by the proposed test; otherwise we should have only one series.

Suppose that we obtained from the application of the skin-reaction test[1] results of the following kind: that the appearance-time of the streak after one hour's muscular work is seven-eighths of the appearance-time before beginning work; that after two hours' muscular work it is seven-eighths of what it was after one; that after three hours' muscular work it is seven-eighths of what it was after two, and so on; and that pauses of certain lengths and at certain times increase the appearance-time by certain definite amounts. What would follow? It would certainly *not* follow that we had a fatigue test *unless we already knew* that fatigue after one hour's work is greater than before work, that after two hours' work it is greater than after one, and so on; and that pauses of certain lengths removed definite amounts of it. If we did not have this knowledge, we should need, after obtaining these results, to discover their significance; and, of course, it *might* then be proved that they indicated the course of fatigue—that the test was a fatigue test. *To prove* this, however, it would be necessary *to know* the degree of fatigue present at any time the test was applied.

Or suppose that some mental task were suggested as a fatigue test. A possible procedure would then be to apply this test to a number of workers at different periods of the day—for convenience, let us say, at 10 a.m. and at 5 p.m. Suppose that 60 per cent. of the subjects were to give a poorer result at 5 p.m. than at 10 a.m., and that the remaining 40 per cent. gave the better result in the afternoon. The experimenter would almost certainly conclude that this mental task is not a test of fatigue. But only if he *knew* that every one of his subjects was more fatigued at 5 p.m. than at 10 a.m., would this be a justifiable conclusion. The chances are considerable that the conclusion would be drawn in the absence of this knowledge, as the degree of fatigue present at any time is frequently assumed. 'Inconsistency of results,' whether obtained from the same or different persons, cannot be used to prove that the method of getting them is, or is not, a test of fatigue, unless it is *known* that different results are obtained when the same degree of fatigue is present, or the same results when different degrees of fatigue are present.

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[1] If a blunt point be drawn across the skin surface, with moderate pressure, a whitish streak appears in a few seconds, which maintains a constant whiteness for a short time and then gradually disappears. Prof. A. H. Ryan, formerly of Tufts Medical School, Boston, has recently attempted to determine whether the duration of this 'skin-reaction' shows characteristic variations with fatigue.

It is now necessary to consider how far the above conditions of experimentation for the finding of a fatigue test are realisable.

II. THE EXTENT TO WHICH THESE CONDITIONS CAN BE REALISED.

(1) In the present state of scientific knowledge, two definitions of the intrinsic nature of fatigue may be offered. When organic matter is 'continuously' active, energy is transformed into heat and mechanical work, and there tend to accumulate chemical products of the activity, of the character of organic poisons. The nature of fatigue may be specified by reference to either of these two facts.

(*a*) Fatigue may be defined as that condition in which a certain percentage of available organic energy has been transformed into heat or work (the more energy transformed after this point has been reached, the greater the fatigue).

Determination of the percentage of energy to be transformed before the fatigue condition is reached would be guided by common usage of the term fatigue. Once this percentage had been determined, the definition would yield an adequate experimental situation. Calorimetry would show when different percentages of energy had been transformed, and consequently when different degrees of fatigue, according to the definition, were present. It would then be possible to search for some unique characteristic of the fatigue condition, and the method of showing the presence or absence of this characteristic at any time would be a fatigue test.

While adequate for the purpose of fatigue test experimentation, this definition does not conform to current scientific opinion concerning the essential feature of fatigue. The current view stresses the accumulation of organic poisons in the active organ. It is based chiefly upon the results of experiments with muscle and nerve-muscle preparation, and is in accord with certain of the facts brought to light by these experiments. We thus come to the second definition.

(*b*) Fatigue may be defined as that condition in which certain chemical products of activity, of the nature of organic poisons, have accumulated in the organ that has been active; 'different degrees of fatigue' meaning 'different amounts of these activity products.' As it is known what some at least of these products are in active muscle, it might seem that this definition would be satisfactory; but this is not the case.

When stimuli are repeatedly applied to nerves, a condition soon occurs at the synapses (at least) that renders the passage of the impulse more difficult. The nature of this blocking is at present speculative; it certainly is not known to result from the accumulation of metabolites. The synapse blocking is nevertheless considered most definitely to be a part of fatigue. Consequently, it is impossible at present to define fatigue by reference to the accumulation of metabolites alone; *the definition adopted must refer both to these and to the phenomena of 'blocking.'*

The question that now arises is: can we (independently of a fatigue test) determine the degree of fatigue present at different times when such a definition is accepted?

It is clear that fatigue in the present sense cannot be directly observed. There is, therefore, only one method by which its presence and its degree at any time can be known, namely, deduction from something else that is observed. Hence, everything depends, so far as our present purpose is concerned, upon whether or not there can be found some phenomenon from which this deduction can be made; and this resolves itself into *the possibility of finding some characteristic expression of fatigue. If such an expression cannot be found, fatigue test experimentation with reference to the intact organism has no sufficiently definite problem.*

(2) The *acceptance* of a characteristic expression of fatigue in the intact organism is included in that *general* definition of fatigue most usually adopted at the present time; according to which *fatigue is a condition caused by work, in which the capacity for work is diminished.* The characteristic expression of fatigue here accepted, and explicitly stated to be such in certain forms of the definition, is *a diminished capacity for work.* We have now to consider whether this is adequate for our purpose.

As usually *conceived,* the general definition just stated requires more precision in certain minor particulars than occurs in this statement of it; and in introducing this, we are clearing the ground for a discussion of the question just raised. Two points arise concerning the term *work*:—

(*a*) The definition might be taken to mean that play, as distinct from work, could not produce fatigue. Such a conclusion would probably be denied by most of those who accept the definition: if capacity for work can be diminished by work, it seems certain that it *can* also be diminished by play. In any case, it is often difficult to determine whether a particular activity is play or work. It may be work in respect of the fact that it yields remuneration, and play so far as the performer's attitude is concerned. This ambiguity in the definition does not occur when the term *activity* is substituted for the term *work.*

(*b*) The definition does not specify the *kind* of work for which capacity is diminished in fatigue. It could be taken to mean either that capacity for *every* kind of work is diminished, or that capacity for the particular kind of work that caused it is diminished. This ambiguity is removed in that statement of the definition which explicitly limits the lowered capacity to the performance of the activity which caused it. And this keeps the definition within the body of known fact. It prevents any premature conclusion as to whether fatigue is general or specific since in both cases it would, according to current conceptions, diminish the capacity for performing the activity which caused it.

(*c*) A statement of the 'diminished capacity' definition of fatigue which would remove the above ambiguities would be as follows: *fatigue is a condition caused by activity, in which the capacity for repeating the activity that caused it is diminished.*

(3) (*a*) The serious difficulty in the 'diminished capacity' definition of fatigue now comes to light. When an attempt is made to determine (independently of a fatigue test) in what degree fatigue is present at any time, so that it can be decided whether a proposed fatigue test is a diagnostic instrument or not, it becomes clear that the expression of fatigue indicated in the definition *is not adequate for our purpose*. The meaning of 'diminished capacity' is not self-evident, though from such a statement as fatigue is *shown and measurable* by it[1], there seems to be a general opinion that it is. Various statements in fatigue literature suggest that 'diminished capacity' can be *observed directly*, and as easily as the shape of a man's nose. But if *any* degree of capacity, small or great, can ever be detected, it is not by directly observing it but by observing something else which is a mark for it—from which it can be inferred. The diminished capacity said to be characteristic of fatigue (in such a statement, for instance, as fatigue is 'shown by' it) can *only* be discovered by discovering some way in which it expresses itself. If diminished capacity is the most characteristic 'expression' of fatigue we know of, then, since it cannot be itself directly observed, we must discover some characteristic way in which *it* expresses *itself* before fatigue test experimentation becomes justifiable.

Briefly, the position is this. Since fatigue, in the present sense, cannot be directly observed, we require (before fatigue test experimentation begins) something observable (an *expression* of fatigue) from which its presence can be inferred. The *accepted* 'expression' (diminished capacity) is itself not directly observable; and, consequently, we require an observable expression of this 'expression' of fatigue, from which the presence of diminished capacity (and hence of fatigue) may be inferred.

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[1] "Real or objective fatigue is shown and is measurable by the diminished capacity for performing the act that caused it." Memo. 7, *Health of Munitions Workers Committee*, "Industrial Fatigue and its Causes," p. 5. Cd. 8213. 1916.

Now, if diminished capacity is an 'expression' of fatigue, any observable expression of diminished capacity can be considered simply as an expression of fatigue. We may therefore speak *as though any expression of diminished capacity* due to prior activity were an *expression of fatigue.*

(*b*) It is perhaps most natural to assume that 'a condition of diminished capacity' is *a condition in which output is relatively poor in quantity or quality, or both*; a definition which may be taken to cover the results of special performance tests as well as industrial output. What is meant by relatively poor output is, or can be made, perfectly clear; and *if* such output is a constant characteristic of a condition of diminished capacity, and *if* diminished capacity is a characteristic 'expression' of fatigue, we have all that is required for our purpose in searching for a fatigue test. *Assuming that these hypothetical propositions are true,* what we need to do is to find some test which yields one type of result when output is poor and another type when output is good, and other but consistent results with intermediate degrees of excellence of output.

(*c*) The supposition that relatively poor output is a characteristic expression of diminished capacity has to meet considerable difficulty. It leads us to assert the absence of fatigue in certain cases when, in common belief, it is most certainly present, and to deny its absence in other cases when, in common belief, it is most certainly not present. If these common beliefs are accepted, it must be admitted that fatigue is sometimes present when output is relatively good and sometimes absent when output is relatively bad; and this admission does not agree with the proposed characteristic expression of diminished capacity. Therefore, *either* that expression *or* these common beliefs about the presence and absence of fatigue at certain times must be given up. While these common beliefs make use of a nebulous conception of fatigue, various considerations suggest that some modification of the above characteristic expression of diminished capacity is necessary. These considerations may be grouped under two heads.

(i) Within certain limits, *output is a function of the intensity of the stimulus to activity.*

This law, called by Dodge "the first law of relative fatigue[1]," may be illustrated from the results of experiments with nerve-muscle preparation. When a stimulus of weak intensity is applied to such a preparation, the muscle contracts and continues to contract for a time with repetition of the stimulus, the excursions presently becoming smaller. Immediately after it has ceased to respond to this stimulus, it will respond with further contractions to stronger stimuli. Moreover, before the point has been reached when the weak stimulus is no longer adequate to evoke a contraction, a stronger stimulus will, within limits, increase the extent of the contractions.

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[1] R. Dodge, "The Laws of Relative Fatigue," *Psychol. Rev.* 1917, xxiv. 102.

A similar phenomenon occurs with regard to the organism as a whole and with respect to those stimuli commonly called *incentives.* The relatively small late-afternoon output of an industrial worker may suddenly become large as a consequence of an appeal to any one or several of a number of 'motives'—to fear, ambition, or patriotism. His relatively large output between 10 a.m. and 12 a.m. may suddenly become small if he be told that his employer is treating him unfairly, profiteering generally, or preparing to cut piece rates. The whole question of systems of wages, so far as they are incentives to work, is here relevant. The phenomena of spurts manifested in continuous work possibly arise from variations in the intensity of incentives from one moment to another, especially if variations in spontaneous *interest* are included, as they should be, among incentives to work[1].

(ii) Within certain limits, *the character of output depends upon the intensity of stimuli to other kinds of activity than that which produces it.*

Illustrations of this fact can be found in the results of experiments upon 'competing' systems of reflex arcs; but it will probably suffice if there are noted here certain experiences of a well-known character.

Even with a superabundance of 'capacity,' it is often difficult to concentrate attention upon some particular work, and at such times output is relatively poor compared with other times when concentration of attention is easy. Irrelevant matters come into the mind, causing distraction; and the occurrence of such distraction is at the expense of both the quantity and the quality of output. It is simply contrary to facts to say that the only time that such distractions occur is when we are fatigued—when 'capacity is diminished.' While they do occur at such times, they occur also, almost universally, at the beginning of a period of work; and their gradual elimination seems what in part is meant by 'warming up,' though doubtless this has a more strictly physiological reference also. To adjust the organism to the task of concentrating attention and activity upon certain 'work' for several hours, necessitates the inhibition of associations which recent experience has created and which involve tendencies to action different from those involved in the particular work. The interference thus occasioned between competing systems may render output relatively poor, and such output may occur *at any time* as a result of such interference. Probably the feelings of weariness, with their positive tendency to cause a general relaxation of effort, are important here; but by no means these only.

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[1] It needs to be emphasized that the operation of mental stimuli is often extremely subtle. The following statement by R. A. Spaeth is of some interest here. Speaking of ergographic work, he says: "If a man has been raising five or eight pounds for some time, he reaches a point where he can't work any longer, but if we remove a pound, the individual finds he can go on. We also find, that when certain individuals are told, 'We are now removing a pound,' but actually we add a pound instead of taking one off, the individual starts merrily on and proceeds to work....The thing I want to bring out is that the suggestion of work and the idea that one is capable of work plays a large part in actual performance. The reverse is quite true as demonstrated by the fact that if I tell the individual that I have added a weight and really remove one, he says he cannot go any further....We have repeatedly carried out this experiment by way of demonstrating that the working capacity is not a fixed base line." *Industrial Education*, p. 204. (The Society of Industrial Engineers, New York, 1920.)

The individual normally experiences impulses of a more or less instinctive nature: and the interference of these with work stimuli will cause output to deteriorate. Up to a certain point, suppressing impulses renders them more insistent. In modern industrial conditions, the worker stands in an environment highly artificial; and after tending a machine requiring constant attention for hours together, he will feel an ever-growing desire to go somewhere and to talk to his fellows. When such desires are strong, they distract attention from work and lower output. The point is, briefly, that such interferences may and sometimes do occur when, according to common belief, fatigue is not present. We know broadly the effects of 'competing stimuli,' and this enables us to say with certainty that relatively poor output is not universally the expression of diminished capacity. It may be due in a specific case to the occurrence of a strong stimulus to a rival form of activity; and such a stimulus may arise from within the organism—it may be an 'impulse.'

(iii) Since, then, output is relative both to its own stimulus and to that of activity incompatible with it, there cannot be an exact correlation between the condition of the organism and the character of output—such as would enable us to say that one particular type of output universally indicates one condition of the organism. The incentives to work and the general organic condition of the worker vary independently of each other: the greatest incentive may be experienced when the organism is least vigorous, and the least incentive when it is most vigorous; and the greatest incentive acting upon diminished capacity may yield an output equal to or better than a very small incentive acting upon great capacity[1]. Output, in such a case as the former, would *not* show fatigue, although fatigue would be present; and, on the other hand, in the absence of fatigue output may be relatively poor because the incentives to some activity incompatible with that required for its production are very strong.

(*d*) The statement that the condition of diminished capacity expresses itself in relatively poor output must therefore be altered by substituting *tends to express* for *expresses* itself. This will mean that it expresses itself in this way *in the absence of interfering factors.*

But a further alteration, indicating that there is a tendency for diminution in capacity and poorness of output to vary directly, must also be made. This is necessary, first, because it is in accord with the results of physiological experiments on fatigue, and, secondly, because we must know that different *degrees* of fatigue are present at certain times before a fatigue test can be established. We shall then have to say: *diminished capacity* is a condition which *tends* to express itself in relatively poor output, the degree of poorness *tending* to vary directly with diminution in capacity. So far as fatigue is concerned, it would be necessary to show in any case that diminished capacity was due to prior activity.

(4) Having seen how the commonly accepted expression of diminished capacity must be modified if from its presence diminished capacity may be inferred, we must consider one other fundamental question. *Is it justifiable to accept diminished capacity as a characteristic expression of fatigue?* So far an affirmative answer to this question has been *assumed.*

There seems no ground for questioning this procedure so long as attention is confined to fatigue in the relatively isolated muscle. A characteristic feature of fatigue in muscle or nerve-muscle preparation is the gradual deterioration of muscle output. A broadly similar result is obtained from ergographic work with a relatively isolated group of muscles, with regard to which, it may be supposed, there occurs a gradual accumulation of metabolites and some blocking of impulse paths. In such cases as these, in which interfering factors can be largely or altogether controlled, diminished capacity (as inferred from its expression) appears to be a constant 'expression' of fatigue. The acceptance of diminished capacity as a characteristic 'expression' of fatigue in the intact organism is an inference from these phenomena. We are unable to say if it is altogether justifiable; but it seems reasonable, provided *its* expression be modified to allow for interfering factors in the manner already outlined.

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[1] The case in which excellent output is obtained in 'serious fatigue' should also be mentioned—whether this be due to the 'failure of higher control' or to other causes. Cf. C. S. Myers, *Mind and Work*, 1921, 46.

It will be observed that no attempt has been made to define the intrinsic nature of *diminished capacity*. The situation is such that no definition of this term is required. In fact, since (1) diminished capacity is itself not directly observable, since (2) it 'expresses' something else (fatigue) also not directly observable, and since (3) in the absence of interfering factors it is itself expressed by something that *is* directly observable, *it may be altogether eliminated from fatigue discussion* which can proceed just as well in terms of *fatigue* and *output*. It would be an aid to clarity of thought if this were done, as, in its present status, 'diminished capacity' is nothing more than an algebraical symbol, and may conceal ambiguity or carry unjustified conclusions.

The definition of *fatigue* would then be as follows: *Fatigue is a condition* (partly[1] specifiable by reference to accumulation of metabolites and blocking in impulse paths) *caused by activity, in which the output produced by that activity tends to be relatively poor; and the degree of fatigue tends to vary directly with poorness of output.*

(5) The experimental situation that now results is far from ideal; but it is possible. We do not know of any absolute constant characteristic expression of fatigue: the most constant expression we know of is evoked sometimes by conditions in which fatigue is not present, while at other times certain factors interfere with its occurrence and thus mask the presence of fatigue.

The possibility of finding a fatigue test thus depends upon the possibility of eliminating or of determining the precise effects of all interfering factors (among which are included factors tending to produce the accepted characteristic expression of fatigue in the absence of the fatigued condition). Among these factors are (*a*) incitement, (*b*) practice, (*c*) spurts, (*d*) illness, (*e*) diurnal rhythm, (*f*) incentives to a given kind of activity, and (*g*) competing incentives. It seems possible to control the first five of these, but impossible to control the two last. Whether it is possible to determine the precise effects upon output of ineliminable interfering factors, is a highly problematical question. The mere discovery that

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[1] A considerable step forward would be made by the exact determination of the *intrinsic* nature of fatigue. "Fatigue is undefined as a physiological entity" (C. K. Drinker, *J. of Industr. Hyg.* 1920, II. 114–115). It is undefined, intrinsically, in *any* terms.

any factor influences output is very important here; and the elimination or determination of the precise effects of each such factor is in itself a considerable experimental problem. This work, however, *must precede* the application of proposed fatigue tests: that is, *the first requirement is a* technique *for determining independently of a proposed test in what degree fatigue is present at certain times*. For the carrying out of this work, the interfering factors must be under the control of the experimenter, and this condition cannot generally be realised in industry. Once this work has been done, the next question concerns the actual fatigue tests to be used.

III. PROPOSED FATIGUE TESTS.

(1) From the points of view of the criticisms which may be urged against them, and the *technique* required for their application, proposed fatigue tests may be conveniently classified under two main heads—*performance* tests and *non-performance* tests.

A *performance test* may be defined as one that requires a subject to do something, the character of what is done being regarded as a possible indicator of fatigue. Thus, an ergographic test is a performance test. A *non-performance test* is one that is directed to the measurement of certain non-voluntary phenomena, the character of which is regarded as a possible indicator of fatigue. Thus, the measurement of blood-pressure is a non-performance test.

Most proposed tests of fatigue are performance tests.

(2) *Performance tests* may be classified, first, as *workshop* tests and *laboratory* tests.

There is one *workshop* performance test, namely the character (quantity and quality) of industrial output.

Of *laboratory* performance tests there is an indefinite number, and these may be classified in various ways. Perhaps the most convenient, if rough, method of classification is by reference to that part of the organism whose performance they seem specially designed to measure. By this method we first have (*a*) *muscular* tests, and (*b*) *mental* tests. These may be again subdivided as follows:

(*a*) *Muscular tests:*
  (i) *Muscular strength tests* (including *endurance* tests); *e.g.*, dynamometer tests, ergograph tests, tapping;
  (ii) *Muscular precision tests*, *e.g.*, the steadiness test, 'pursuit pendulum[1],' dart-throwing;

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[1] See W. R. Miles, *Psychol. Rev.* 1920, XXVII. 361–376.

  - (iii) *Muscular rapidity tests, e.g.*, tapping.
- (*b*). *Mental tests:*
  - (i) *Algometer tests;*
  - (ii) *Tests of sensory acuity;*
  - (iii) *Tests of immediate memory;*
  - (iv) *Tests of attention;*
  - (v) *Tests of apprehension;*
  - (vi) *Tests of judgment and reasoning.*

In addition, there is one test, at least, which does not readily fall under any of these heads, namely, *the reaction-time test.*

An indefinite number of tests may be devised under each of the above sub-headings.

(3) Proposed *non-performance* tests of fatigue may also be classified as *workshop* and *laboratory* tests.

The only possible *workshop* non-performance tests are two, *industrial accidents* and *consumption of power. Industrial absenteeism, sickness* and *labour turnover* have also been suggested as possible tests; but while these phenomena are almost certainly influenced by fatigue, the other factors which may influence them are so many and potent that it is impossible to consider them seriously as *tests.*

The number of proposed *laboratory* non-performance tests is small. The chief that have been suggested are the following eight:

(*a*) Variations in blood-pressure;
(*b*) " " the pulse;
(*c*) " " the 'skin-reaction'[1];
(*d*) " " physiological reflexes;
(*e*) " " psycho-galvanic reactions;
(*f*) " " excretions;
(*g*) " " muscle-tonus;
(*h*) Calorimetry.

(4) The question now to be considered is whether some of these tests are more promising than others.

(*a*) *Performance tests.* Certain general considerations necessitate the surrender of *all* performance tests as possible tests of fatigue. They are not, as a rule, convenient tests; and none of them can yield results that do not require interpretation and correction for interfering factors. This may be made clear briefly.

All performance tests are affected by the factors already shown to interfere with the accepted characteristic expression of fatigue. So far as these tests are concerned, therefore, these factors are interfering factors, whose influence must be eliminated before a result can indicate fatigue unambiguously. Such an elimination seems possible, though difficult, when the performance test is continuous over a long period, or even if, in itself occupying a short period, it can be given to groups or individuals many times; but it is possible neither to eliminate all these factors nor to determine their influence at any one application of the test, and especially at its first application. A convenient and rapid test (and the idea of a test includes rapidity) would be one which need be applied to an individual or a group on a few occasions only in order to indicate the degree of fatigue present. But it takes time to eliminate the influence of practice or warming up or spurts; and it is impossible to determine the effects of fluctuations in incentives to activity, which may altogether mask the presence of fatigue, in a period of short duration—if indeed, it is possible in such a period to know that they occur. Spontaneous fluctuations in interest, for instance, or in the desire to do one's best, to mention no other factors, are entirely outside the control of the experimenter and most largely remain outside his knowledge: the most that can be hoped is that their influence may be isolated in the results of performance tests carried out for a long period. But this means that such tests cannot be rapid tests. This conclusion gains confirmation from what is commonly called the 'inconsistency' of the experimental results obtained from the application of performance tests.

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[1] See footnote, p. 32.

Performance tests, therefore, must be altogether given up if experimentation with them is undertaken in the hope of finding among them a *rapid and convenient* fatigue test. If used for other purposes, however, they may yield valuable results. For it is only by means of performances that it can be known, in the first instance, in what degree, if any, fatigue is present at certain times. Hence, experimental work with non-performance tests *must* be combined with a consideration of performance results (in order that the worth of the non-performance tests may be rightly judged). For this purpose almost any kind of performance should be valuable. In practice, the reliability of a proposed non-performance test would need to be tested by reference to the results of various types of performance.

(*b*) *Non-performance tests.* Certain of those factors previously designated *interfering* factors in the case of *performance* tests are not interfering factors in the case of *non-performance* tests. The results of a *non-performance* test are practically unaffected by practice, for instance. It might therefore seem that there is considerable probability of obtaining a rapid and convenient fatigue test of a non-performance type. Two points, however, should be noted.

First, there might in any case be no rapid and convenient fatigue test, because no uniquely characteristic mark of the fatigued condition might be brought to light by any non-performance test.

Secondly, it is a mistake to suppose that the results of non-performance tests are unaffected by factors other than fatigue. Sickness may interfere with the result of a non-performance test just as much as with the result of a performance test. Accidents may be caused by factors among which fatigue has no place. All circulatory tests (the first three of the laboratory non-performance tests) are directly influenced by emotions, and it is impossible for the experimenter to control these, or to eliminate their influence except in results covering long periods; which means that circulatory tests cannot be rapid tests unless we possess also a rapid test of the presence of emotions, and a knowledge of their circulatory effects which could then (theoretically) be allowed for. Again, although experimentation with psycho-galvanic reactions is in its infancy, it shows that 'emotive reactions' are influenced by other factors in addition to fatigue, if fatigue *has* a characteristic effect upon them.

Concerning the other proposed non-performance tests, all that need be said here is that such facts as are known concerning the nature of excretions and calorimetry suggest that these will yield no test of fatigue; that at present there is no satisfactory method of measuring muscle tonus; and that the possibilities of physiological reflexes as a test are not clear.

(5) It is worth noting that the mere demonstration of the presence of fatigue at a given time has no practical significance whatever. The practical question asks at what point fatigue becomes inimical to efficiency and to health. Consequently, if a test is to be valuable practically, it must be capable of discriminating nicely between degrees of fatigue. When such a test is obtained, extensive inquiries would need to be undertaken to determine the relation between these different degrees of fatigue, on the one hand, and efficiency and health, on the other hand. It might then be possible to say, first, that such and such a degree of fatigue (or a greater degree) is bad; and, secondly, that we have a means of knowing when any degree of fatigue inimical to health or efficiency is present. If we cannot finally make such statements, our fatigue tests will have no *practical* value whatever.

IV. Conclusions and Suggestions.

(1) An essential pre-condition of experimentation designed to obtain a fatigue test is the knowledge that different degrees of fatigue are present at certain times. This knowledge can probably be obtained *if* it be accepted that fatigue tends to express itself in relatively poor output. It would then be necessary to develop a *technique* such that by it either (*a*) factors which interfere with this characteristic expression of fatigue can be eliminated, or (*b*) the presence of interfering factors can be known and their effects upon output precisely determined.

If this *technique* could be obtained we should possess *in it* a fatigue test[1], that is, a method by which it could be shown in what degree, if any, fatigue was present at given times, but we should not have a rapid and convenient fatigue test.

(2) Given any method by which we can determine in what degree, if any, fatigue is present at certain times, it is justifiable to experiment with the object of finding a rapid and convenient fatigue test. We should proceed largely by the method of trial and error, our object being to find *some* test that would rapidly yield at any time a characteristic result if a given degree of fatigue were present. The only suggested tests that could possibly yield such a result are non-performance tests; and consequently experiment aiming to discover a rapid test of fatigue must work with such tests. If such a test were found, the degree of fatigue present at any time could be determined without a lengthy and difficult analysis of output figures.

(3) In view of the foregoing considerations it is recommended that the whole fatigue test problem be stated in a form the nature of which may be indicated by the following suggestions:

(*a*) That the term *fatigue* be absolutely banished from precise scientific discussion, and consequently that attempts to obtain a fatigue *test* be abandoned.

(*b*) That the problem to be investigated be defined as the determination of the effects of different kinds and amounts of work (activity) upon mental and physiological functions: that is, that the kind and amount of work be correlated *directly* with changes in psycho-physiological functions, and *not* (as at present) *indirectly* by means of 'fatigue.' The various proposed fatigue 'tests' would then be used to determine *effects of activity* and not *the presence or absence of fatigue.* Thus stated, 'fatigue investigation' seems to offer a fertile field for scientific work.

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[1] This is only true *on the assumption*, stated under II. (4), that diminished capacity (expressed in relatively poor output) is a characteristic expression of fatigue. Without this assumption we are left with only one series—that constituted by output of different degrees of excellence. It is only by assuming that such a series runs parallel with fatigue that we obtain the two series, a knowledge of which was stated in I. (*b*) to be essential.

(*c*) That, so far as practical purposes are concerned, attention should be concentrated on methods, such as motion study, by which the amount of *work* required for a given quantity of output can be decreased.

A PRELIMINARY STUDY OF THE REPRODUCTION OF HAND MOVEMENTS.

By C. RODRIGO LAVÍN.

(*From the Cambridge Psychological Laboratory.*)

1. *The Problem* (p. 47).
2. *Apparatus* (p. 47).
3. *Method* (p. 48).
4. *Results* (p. 49).
5. *General Suggestions* (p. 51).

§ 1. The Problem.

The general purpose of the experiments, the results of which will here be briefly discussed, was to determine the most favourable conditions for the learning and reproduction of hand movements. The practical bearing of this problem upon various educational and industrial questions is obvious, while it also raises interesting theoretical points, such as the relation between visual and motor perception of space, or between visual and motor imagery.

Six subjects were employed, five men and one woman, all of whom were of adult age. Each subject executed the prescribed movement in a manner which will be described later, and after a lapse of five minutes he attempted his reproduction of the movement. This was the procedure adopted at the first sitting. In subsequent sittings, if further practice in the execution of the movements was required, the same course was adopted. The total time occupied at any given sitting was not more than one hour, this limit being imposed so as to avoid fatigue effects.

I wish at the outset to thank Mr F. C. Bartlett, who proposed the subject of this investigation and helped to prepare this paper for publication.

§ 2. Apparatus.

The apparatus used consisted of a metal sheet 47 cm. × 39 cm. divided into 20 7½ cm. squares, the sides of these squares being made by lines of sealing-wax let into the metal sheet, and level with its surface. These sealing-wax boundaries were broken at the corners of the squares, so that no portion of the metal sheet was completely isolated. The whole sheet was placed in circuit with a metal pointer, a steel pen, and two electric time signals, and the latter were made to register, in the usual way, upon a travelling paper surface, below a Jacquet chronometer. Simple curved patterns were cut out of sheets of thin wood, which were then placed over the metal background. The subject traced the pattern with the metal pointer, following the path cut in the wooden model. The model was then removed, and after the prescribed interval, the subject attempted, with the steel pen, to reproduce the movement on the same metal background. A copy was taken on tracing paper of his attempt, the whole sponged off, and the process repeated. The passing of the pointer or pen over a line of sealing-wax caused a break in the current flowing through the time-signal, and thus the total time occupied by a complete execution or reproduction could be readily computed, and also the time taken by various sections of the movement.

§ 3. METHOD.

The general plan was to vary, one at a time, different specified conditions of learning and of reproduction. Thus the following conditions were imposed:

Learning		Reproduction		
L. hand or R. hand	eyes closed	eyes closed	L. hand	R. hand
		eyes open	”	”
R. hand or L. hand	eyes open	eyes open	”	”
		eyes closed	”	”

Again the subject learned by watching the experimenter perform the movement, his reproductions being effected in the various modes specified above. Or in some cases the effect of previous practice was observed by instructing the subject to draw any meaningless pattern he pleased. Some attempt also was made to study the effect of eye movement, but this was not very successful, and will not be further discussed. In every case a reproduction was attempted after *each* practice movement.

The general procedure was as follows:—

(*a*) the subject executed the movement by the help of the pattern-board, being allowed always to do so at his own rate of speed;

(*b*) an interval of five minutes was imposed, this interval being filled by some other task, or by conversation;

(*c*) the subject attempted his reproduction in the prescribed manner.

Five separate executions and reproductions completed a single sitting, and the sittings were continued with each subject for each pattern employed until the reproduction was accurate. The total number of sittings required by different subjects varied considerably.

§ 4. RESULTS.

No attempt will be made to discuss exhaustively the results obtained. It is recognised that further subjects and more experimentation are needed. A number of the conclusions suggested however seem to be of general interest, and these will be briefly indicated:

(i) In the early stages of practice all of the subjects attended to the *form* of a movement rather than to its *extent*. Thus one who, after a series of 15 trials, each trial followed by a reproduction, was asked if he clearly recalled the drawing, replied "I know its form well enough, but not its size." The inability to remember extent during the early stages of a reproduced movement is most marked of all, when the first execution is made with the eyes closed. There seems to be no doubt that the normal mode of acquisition of a new 'meaningless' hand movement is first to learn the form, and only afterwards the size. Satisfaction on the part of the subject with his reproduction came only when the size was being learned; and it was often noticeable that, although the subject was very dissatisfied with his attempt, his reproduction as regards the details of form was fairly accurate.

(ii) The points of movement most speedily and accurately learned were the beginning and end (as in the case of a series of nonsense syllables), and wherever sharp changes of direction occurred.

(iii) If the eyes were closed during learning, or reproduction, or both, the acquisition of the movement was retarded. In particular, subjects who reproduced with closed eyes very readily interchanged the dimensions of two curves in sequence, or the directions of symmetrical curves. These errors were especially liable to occur at the beginning of the movement.

(iv) Different forms of hand movement were very readily coalesced, or 'condensed.' Thus when a subject had been practised on a certain movement, and was then set to reproduce a different one, the result was invariably at first a combination of the two. Again when a subject, practised in a certain form of movement, was told to draw haphazard meaningless patterns differing from one another as widely as possible, it was astonishing to observe the general likeness of his attempts to the practised movement.

(v) When, in the original execution, the subject's hand was guided by the experimenter, the process of learning was very distinctly retarded.

(vi) The rate of learning was not affected according as the right or left hand was used. Moreover if practice was given to one hand, nevertheless reproductions could be equally well effected by the other hand; and, as regards their general form at least, reproductions could be correctly effected also by other parts of the body, *e.g.* the head.

(vii) Individuals differed as to their relative abilities in reproduction with eyes open or closed. But the influence of this factor was independent of the method of learning.

(viii) When a number of similar curves followed one another, the reproductions were several times in error concerning the precise number of curves reproduced. Errors of this character proved to be unusually difficult to eradicate, and often persisted even when practice was given with sight.

(ix) The most favourable method for the teaching of movements of the kind here studied, was for the experimenter to make the tracing and for the subject to watch him. The results were quite unequivocal, and would appear to have important general bearings upon practice; but further observations on different types of movement are required. As already mentioned, subjects showed consistent individual differences as to the relative accuracy of reproduction with eyes open or closed. Making allowance for this, the methods of practice may be arranged in the following order, as regards their efficiency:

		Movements reproduced	
	Movements learned by	by some subjects with	by other subjects with
1.	watching them done by another	eyes open	eyes closed
2.	performing them with eyes open	,, open	,, closed
3.	watching them done by another	,, closed	,, open
4.	performing them with eyes open	,, closed	,, open
5.	,, ,, ,, closed	,, open	,, closed
6.	,, ,, ,, closed	,, closed	,, open

(x) As regards the time of the reproduction in relation to the time of a practice movement, the subjects gave consistent results as follows:

Form of practice	Time spent in reproduction
With eyes closed	Less than in practice
Watching movement performed by another	Variable: very little difference
With eyes open	More than in practice

(xi) Movements learned and reproduced with the eyes closed showed a striking diminution in the size of the reproductions. If the eyes were allowed to be open during one only of the two periods of practice and reproduction, the size was still commonly diminished, but less markedly so. When the subject watched the movements as they were performed by another and then reproduced them with his eyes shut, the reproduction was still of a smaller size, but in a slight degree only. Again, when a subject was allowed to learn a movement thoroughly by dint of much practice, and was then asked for two reproductions, one with his eyes closed and the other with his eyes open, the two attempts were alike as to form, but the first was invariably smaller than the second. Further experiments should be carried out with regard to the judgment of distance or size based upon motor as compared with visual experience.

§ 5. GENERAL SUGGESTIONS.

These results appear to open up a large field of study and to justify a few general suggestions.

(*a*) Clearly the adult subjects employed in these experiments brought to their task a variety of preformed habits of observation. This makes it hard to state exactly what were the main bases of their attempts at reproduction of the movements. Not only were there muscle-joint-tendon sensations, and sensations of cutaneous origin, but it is certain that visual and verbal images were often of the greatest use. The very fact, for example, that movements learnt with the left hand appeared equally well traceable with the right suggests that some spatial scheme expressed in terms other than those of local motor sensations must have been used. It would be interesting to know whether equal facility of transference would be found in subjects blind from birth. Similarly a study should be made of subjects who have become anaesthetic as regards motor and cutaneous sensation, and experiments should be specially devised to test the possible effects of eye movement.

(*b*) A matter of the greatest practical interest is that of the basis of the superiority of watching a movement over merely performing it as a method of teaching. Is this equally great for all types of subject, and for all kinds of movement? It may be the case that the superiority is maintained only when the watched movement is performed so slowly that its analysis is easy.

(*c*) The diminution of size which marked all reproductions carried out without the aid of vision is a matter which clearly calls for further study. That the results of the present experiment were not merely accidental would appear to be certain from the fact that in recent observations carried out on children's reproductions of simple forms exactly the same diminution of size was practically invariably secured.

It may be that in teaching any new form of skilled movement the most important factor for the instructor to lay stress upon is the *extent* of the movement.

(*d*) It may be that in all reproductions of movement carried out upon a basis of motor experience only, 'points of reference' are developed which are in the main determined by changes in direction in the complete pattern of movement in question. Thus, in the present case, when several successive attempted repetitions of the same movement were traced on the same background, it was very noticeable how the whole of the attempts tended to come together at certain points. These points seem to have the function of common 'points of reference' between which successive movements may diverge considerably, but at which all attempts join. Such 'points of reference' may be regarded, perhaps, as sections of a complete movement which are unusually readily established. They seem to bear a definite relationship to changes of direction of a curving movement. Thus they may all be regarded in a way as starting-points, and the ready establishment of starting-points for 'muscular memory' seems to yield a perfect analogy with the favourable position of first elements in a series for 'verbal memory.'

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SUGGESTIBILITY WITH AND WITHOUT PRESTIGE IN CHILDREN.

By F. AVELING and H. L. HARGREAVES.

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I. *The first research* (pp. 53–63):
  (1) *Description of the tests, with frequency distribution tables.*
  (2) *Correlations between the various tests.*
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  (1) *Frequency distributions for the various tests.*
  (2) *Correlations between the tests.*
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  (1) *Comparison of the frequency distributions reached in the two researches.*
  (2) *Discrepancies in the results.*
  (3) *Comparison and summation of the correlations.*
  (4) *The problem of a 'general factor.'*
IV. *Summary* (p. 75).

I.

The first of the researches upon which the following paper is based was carried out, by the first of the above authors, after some preliminary experiments with the tests, in the autumn term of 1915. The subjects were school children: 27 boys (average age 13) in one school (*A*); and 19 boys (average age 12), and 19 girls (average age 12) in another (*B*).

It was then intended to prolong the experiments with a greater number of subjects, but circumstances made this impracticable at the time; and it was found impossible to continue the research up to the period at which the second investigation was begun. The protocols, however, though few in number, have yielded results which appear to be worth consideration; and have been notably corroborated by the second research. Such evidence as is forthcoming seems to point to a factor of general suggestibility, rather than to specific suggestibilities, as previously believed.

TESTS: Several tests of suggestibility were chosen, of which some had been used in previous work. Seven tests in all were employed. Four only were used with the subjects in school *A*, but all seven with those in school *B*. The following were the tests:

1. Hand Rigidity by Suggestion.
2. Suggestion by Progressive Lines[1].
3. Suggestion by Illusion of Warmth[2].
4. Hand Levitation by Suggestion.
5. Suggestion by Progressive Weights[3].
6. Fidelity of Report Method "to induce a strong degree of suggestion[4]."
7. Suggestion of Line Lengths by Personal Influence; A. Contradictory Suggestion[5].

It will be perceived that these tests fall into two classes: (i) tests without personal prestige, or 'depersonalised' suggestibility tests: and (ii) tests with personal prestige derived from the experimenter. Nos. 2, 3 and 5 would seem to be of the former, Nos. 1, 4, 6 and 7 of the latter class; but results obtained in both researches have led the authors to a modification of this classification. This point will be considered in Section III.

1. *Description of the tests, with frequency distribution tables.*

Test 1. Hand Rigidity by Suggestion.

This test was used with subjects of both schools *A* and *B*.

Method. The subject was seated opposite the experimenter and given the following instruction: "Hold out your hand—so, like mine; and fix your eyes on my hand. Whatever I say, do not take your eyes off my hand. But, while looking at my hand all the time, I want you to pay special attention to the *feelings* you get in your own. Do you understand? You are to look at my hand all the time. Don't look at your own, or at me; but pay attention to the 'feel' of your own hand." During this general instruction the experimenter held out his hand, palm uppermost, easily and loosely, with the fingers very slightly flexed, in a position some four inches above his right knee, so that the subject could easily fixate it. The instruction then continued: "You will probably feel a slight tingling, or tightening, or stiffening in your fingers. I want you to tell me *at once* when you feel this." The moment any such feeling was reported, the stop-watch was started; and the experimenter, at the same time deliberately straightening out his fingers, and very slowly making his hand stiff and rigid, continued: "Now! You will find that tingling and stiffening increase, so that your hand becomes *quite* stiff; so that you can't bend your fingers at all; or close your hand. Do you feel it? It's getting much stiffer now—quite stiff and rigid. Now you *can't* move your fingers. You can't *close* your hand. You can't close it at all. Try!"

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[1] Whipple, *Manual of Mental and Physical Tests*, Test 42.

[2] "Resistance-wire Method," Whipple, *ibid.*, Test 44.

[3] Whipple, *ibid.*, Test 41.

[4] Whipple, *ibid.*, Test 32, Variations of Method (3).

[5] Whipple, *ibid.*, Test 43.

This test was performed once with each child of the two schools *A* and *B*. The results were estimated (i) as to the success in inducing the rigidity, (ii) as to the time interval required for the effect. With regard to the first head, the subjects were graded, in order of suggestibility, in one of the following classes:

(i) Subject tries, but cannot close his hand at all or flex his fingers.
(ii) Subject cannot close his hand, but is able to move his fingers slightly (flexing or lateral movement).
(iii) Subject cannot close his hand, but is able to move his fingers considerably.
(iv) Subject cannot close his hand completely, but can close it partly.
(v) Subject can close his hand, but only slowly and with evident effort.
(vi) Subject reports that his hand is stiff (sometimes that he is unable to close it) but actually does close it without apparent effort.
(vii) Subject closes his hand easily, with little report of stiffness.
(viii) Subject closes his hand easily, with no reported feelings.

The character and extent of the effect of the suggestion would appear in general to be a better criterion of suggestibility, especially in this test (and in No. 4), than the length of the time interval, which would presumably measure rather quickness of accepting the suggestion than suggestibility. A long time interval might be present, for instance, in a subject slow to understand the instruction or to react. The command "Try!" (to close your hand) was given when the experimenter believed, from external signs, that the suggestion had been taken. The first class, however, was divided into two on the ground of length of time interval—under 30 or over 30 seconds. This procedure was justified by the fact that, in all other circumstances, the cases were, as far as could be estimated, entirely similar. We thus get a grading of nine classes for this test. As it was only performed once with each subject, there was on possibility of obtaining a reliability coefficient.

The frequency distribution for the subjects is as follows:

Table 1.

Class...	1	2	3	4	5	6	7	8	9
B. Boys	6	3	1	.	2	.	7	.	.
Girls	2	1	1	.	3	1	9	1	1
A. Boys	.	7	.	.	6	.	4	.	10
Total	8	11	2	.	11	1	20	1	11

This table is interesting in showing how the cases tend to be grouped at each end of the scale. If we divide the subjects into (i) those who can, and (ii) those who cannot, close their hand, we get the following proportion: 46 per cent. Boys, 63 per cent. Girls, Total 51 per cent., who can close their hand; and 54 per cent. Boys, 37 per cent. Girls, Total 49 per cent., who cannot.

Test 2. Suggestion by Progressive Lines.

This test was used only with subjects of school *B*. The 'yields' to suggestion—in the shape of a judgment of increase of length of lines after the fifth—were counted as one each. For each line pronounced 'shorter' than the preceding one, one-half was deducted. Judgments of equality were ignored. This test was only applied once; and subjects were graded in rank according to the marks obtained. The total number of possible suggestions (increases) was fifteen. The frequency distribution for the 38 subjects is as follows:

Table 2.

'Longer' judgments = +1[1]
'Shorter' „ = −½

Marks	15		14		13		12		11		10		9		8		7		6		5		4		3		2		1		0
Boys	.	.	.	.	.	.	.	.	.	1	1	.	.	.	1	1	1	.	1	1	4	3	2	2	.	.	.	.	.	1	.
Girls	.	.	.	.	.	.	.	.	.	.	.	.	.	.	1	1	1	.	3	1	2	3	2	1	.	1	2	.	.	.	1
Total										1	1				2	2	2		4	2	6	6	4	3		1	2			1	1

Test 3. Suggestion by Illusion of Warmth ("Resistance Wire Method").

This test was used with all the subjects of schools *A* and *B*. The apparatus was as described by Whipple ('Special Warmth Tester'), with the exception that the lamps were small ones (6 c.p.) lighted by eight-volt accumulators conspicuously exhibited with prominent wiring upon the table supporting the apparatus. The test was performed ten times with each subject alternately, five times with the right and five times with the left hand. Reaction times were taken with the stop-watch. All subjects were graded in rank for (i) the right hand, and (ii) the left hand, according to the average times taken for warmth to be perceived. (All cases where objective warmth had to be given were reckoned as 60 seconds.) The reliability coefficients were then ascertained[1] (school *A*, right *vs.* left hand $r = \cdot 96$; school *B*, right *vs.* left hand $r = \cdot 96$). The reaction times for both hands were then pooled and averaged; and the subjects were again graded in ranks for correlation with their ranks gained in other tests. A distribution for all amounts of time from 0″ to 60″ (beyond which failure was reckoned) was possible. For present purposes of comparison, set out in 5″ intervals, the frequency for the 65 subjects is given in the following table:

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[1] The subtraction of ½ for 'shorter' judgments is arbitrary. The effect of subtracting 'one' is to leave the form of distribution the same, but to reduce slightly the number of marks obtained, and to give a few subjects 0 or – marks. The same applies to the other tests where ½ has been similarly deducted.

Table 3.

Time (secs.)	0–5	5–10	10–15	15–20	20–25	25–30	30–35	35–40	40–45	45–50	50–55	55–60	60
B. Boys	5	5	4	0	0	0	2	1	0	0	0	1	1
Girls	5	5	2	3	1	1	0	0	0	0	0	0	2
A. Boys	5	3	2	0	3	1	0	0	1	1	0	4	7
Total	15	13	8	3	4	2	2	1	1	1	0	5	10

If it is taken into consideration that the long reaction time averages of this Table (in the neighbourhood of 60″) are calculated from data in which failures predominate, while one or two reactions of nearly 60″ only were obtained, the distribution will be seen to be very similar in character to that shown in Table 1.

Test 4. Hand Levitation by Suggestion.

This test was only applied to the subjects in school *B*. It was of a similar general character to test No. 1. A preliminary instruction was given as follows: "Place your hand on the table so." (Subject, facing experimenter, placed his right hand, palm down, upon the table.) "I am going to lay my hand lightly on top of yours; and I want you to pay very special attention to the feelings you get from your hand. You need not tell me anything about them unless I ask you. I just want you to get the feel of them." The experimenter then laid his hand firmly but lightly upon that of the subject, so that the tips of his fingers rested upon its back near the wrist.*

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[1] Formula: $r' = 1 - \frac{6\Sigma(2d^2)}{4n(n-1)}$. Cf. Spearman, "Footrule for measuring correlation," This *Journal*, II. 89 *sq.*

He continued the instruction: "You will feel your hand growing lighter, as if it was losing weight, and not resting on the table. It gets lighter still now, as if it had no weight at all. Do you feel it?" If the subject answered "No," the instruction was continued in the same sense; if "Yes" the experimenter almost imperceptibly raised his hand, relaxing the very slight pressure and at the same time saying: "Good! It is quite light now. It has no weight at all. It is leaving the table altogether—coming right off it and rising in the air." Reaction times were taken with the stop-watch, from the beginning of the instruction after *.

This test was performed twice, once with the subject standing (in which case it was suspected that a certain amount of pressure by the hand upon the table might have interfered with the success of the suggestion), and once sitting, with the hand and arm to the elbow lying loosely upon the table. The subjects were graded in order of suggestibility for each performance into four classes according to the following criteria:

1. Hand completely off the table (an inch or so), pushing experimenter's hand up: Evaluation = + 2.
2. Hand just off the table (less than ½ inch): Evaluation = + 1.
3. Phalanges much flexed from the table, but finger tips (or some) and/or base of hand at wrist still touching table: Evaluation = − 1.
4. Hand still entirely resting on table: Evaluation = − 2.

Reaction times were not used for purposes of classing the subjects. The reliability of the two divisions of the test was ascertained. It was: for the Boys, $r = {\cdot}64$; for the Girls, $r = {\cdot}99$ (combined $r = {\cdot}81$). The results were then pooled and a fresh grading in rank made for purposes of comparison with the other tests. All the marks gained were added for the final ranking, thus providing a possible nine classes. The suggestibility estimated as above shows the following frequency distribution for the 38 subjects:

Table 4.

Class	1	2	3	4	5	6	7	8	9
Boys	5	.	.	1	1	2	.	6	4
Girls	6	3	1	.	3	.	3	3	.
Total	11	3	1	1	4	2	3	9	4

The distribution here is seen to take a form similar to that in tests Nos. 1 and 2. Dividing the subjects into two groups (i) those whose hands rose from the table, and (ii) those whose hands did not so rise, we obtain:

Table 5.

	Hands off table	Hands not off table
Boys	26 %	74 %
Girls	52 %	48 %
Boys and Girls	40 %	60 %

Test 5. Suggestion by Progressive Weights.

This test was performed with all the subjects of the two schools *A* and *B*, as well as with the two variations of method noted in Whipple. The yields to suggestion ('heavier' judgments, after the first four) were counted one each, and for each 'lighter' judgment one-half was deducted. Judgments of equality were not counted either way. According to the value of his performance, each subject was graded in his rank for each variation of the test, and reliability coefficients were ascertained between the three markings. These are: School *A*, Method 1 with Method 2, $r = \cdot 98$; 1 with 3, $r = \cdot 95$; 2 with 3, $r = \cdot 93$. School *B* (Boys and Girls) 1 with 2, $r = \cdot 67$; 1 with 3, $r = \cdot 48$; 2 with 3, $r = \cdot 52$. The marks for each subject were then pooled, and the subjects graded afresh in rank for purposes of correlation with the yields of other tests. The possible number of suggestions in the pooled tests was 30. The frequency distribution is shown for the 65 subjects in the following table:

Table 6.

'Heavier' judgments, +1. 'Lighter' judgments, $-\frac{1}{2}$.
All cases of $\frac{1}{2}$ are reckoned belonging to the next lower whole number.

Possible suggestions...	30	29	28	27	26	25	24	23	22	21	20	19	18	17	16
B. Boys	.	.	.	1	.	.	.	.	2	.	2	1	2	.	1
Girls	.	.	1	.	.	.	.	1	.	.	.	.	2	1	1
A. Boys	.	.	.	.	.	1	1	.	.	.	.	1	2	.	.
Total	.	.	1	1	.	1	1	1	2	.	2	2	6	1	2
Distributed in 5's	2					5					13				

Possible suggestions...	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1	0
B. Boys	1	2	2	2	1	.	.	.	1	.	.	1	.	.	.	.
Girls	1	2	5	1	1	.	1	1	.	1	.	.	.	.	.	.
A. Boys	2	.	1	1	.	2	2	2	5	1	2	2	1	1	.	.
Total	4	4	8	4	2	2	3	3	6	2	2	3	1	1	.	.
Distributed in 5's	22					16					7					

Test 6. Fidelity of Report.

This test was used with children of school *B* only. Two coloured pictures (postcards—'The Mill' and 'The Village') were used for the test. An interrogatory was drawn up in which a certain number of questions were inserted of an 'expective' and 'implicative' character; as "What kind of animal was walking beside her?" (when there was none); "From which chimney, right or left, was smoke coming?" (when no chimney was seen in the picture). Marks for suggestibility were given as follows: (i) for an answer showing that the subject had taken the suggestion that something was shown in the picture, which *de facto* was not there, (*a*) when such a thing could reasonably be expected to be in the picture (as, *e.g.* chimneys on the house) = 1; (*b*) when there was no reason to suppose such a thing to be in the picture = 3; (ii) when affirmative answers were given to questions such as "Wasn't...there?" "Do you remember...?" = 2; (iii) for further answers determining the colour, position, etc. of something not shown in the picture, but admitted in a previous answer = 1.

The subjects were graded in rank for each picture-test and the reliability coefficient taken ($r = \cdot 50$). The marks were then pooled and the subjects again graded in rank for comparison with the other tests. The possible values of suggestibility, according to the system of marking employed, lay between 0 and 42. The frequency for the 38 subjects is shown in the following table:

Table 7.

Suggestibility	42	41	40	39	38	37	36	35	34	33	32	31	30	29	28	27	26	25	24	23	22	21
B. Boys	.	.	.	.	.	.	.	.	1	.	1	.	.	.	.	.	.	.	1	1	.	.
Girls	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	2	.	.
Total	.	.	.	.	.	.	.	.	1	.	1	.	.	.	.	.	.	.	1	3	.	.
Dist. in 5's	.		.					2					.					4				

Suggestibility	20	19	18	17	16	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1	0
B. Boys	2	.	.	.	1	1	1	1	.	.	1	1	3	1	.	1	.	1	.	1	.
Girls	1	.	1	1	.	.	.	1	.	1	1	2	2	2	3	1	.	1	.	.	.
Total	3	.	1	1	1	1	1	2	.	1	2	3	5	3	3	2	.	2	.	1	.
Dist. in 5's	6					5					16					5					

Test 7. Suggestion of Line Lengths by Personal Influence.
(Contradictory Suggestion.)

This test was employed with subjects in both schools *A* and *B*. It does not appear to be a very suitable one for the purpose; or else the method of employing it in the present research may have been at fault.

The possible number of suggestions was six. The frequency distribution is as follows:

Table 8.

Suggestions	6	5	4	3	2	1	0
B	.	1	3	2	5	9	18
A	.	.	.	.	4	5	18
Total	.	1	3	2	9	14	36

2. *Correlations between the various tests.*

The subjects of both schools *A* and *B* having been finally graded in ranks for each test, the correlations between the various tests (with their probable errors) were ascertained. The method of ranks was used throughout. The correlation coefficients are shown in the following tables (P.E. shown in brackets):

Table 9. (School *B*.)

	1	2	3	4	5	6	7
1. Hand rigidity	.	·43	·40	·30	·28	·28	·04
2. Progressive lines	·43 (·093)	.	·33	·24	·32	·23	–·06
3. Warmth illusion	·40 (·096)	·33 (·101)	.	·17	·10	·05	–·18
4. Hand levitation	·30 (·104)	·24 (·107)	·17 (·110)	.	·07	·01	·12
5. Progressive weights	·28 (·105)	·32 (·101)	·10 (·112)	·07 (·113)	.	·09	·34
6. Fidelity of report	·28 (·105)	·23 (·107)	·05 (·113)	·01 (·114)	·09 (·113)	.	–·03
7. Line lengths	·04 (·114)	–·06 (·113)	–·18 (·109)	·12 (·111)	·34 (·100)	–·03 (·114)	.

Table 10. (School *A*.)

	1	2	3	4	5	6	7
1. Hand rigidity	.	.	·66	.	·23	.	·32
2. Progressive lines	.	.	.	.	.	.	.
3. Warmth illusion	·66 (·078)	.	.	.	·18	.	·18
4. Hand levitation	.	.	.	.	.	.	.
5. Progressive weights	·23 (·127)	.	·18 (·130)	.	.	.	·13
6. Fidelity of report	.	.	.	.	.	.	.
7. Line lengths	·32 (·119)	.	·18 (·130)	.	·13 (·133)	.	.

Table 11. (Schools *A* and *B* weighted.)

	1	2	3	4	5	6	7
1. Hand rigidity	.	.	·51	.	·26	.	·16
2. Progressive lines	.	.	.	.	.	.	.
3. Warmth illusion	·51 (·064)	.	.	.	·13	.	–·03
4. Hand levitation	.	.	.	.	.	.	.
5. Progressive weights	·26 (·081)	.	·13 (·086)	.	.	.	·25
6. Fidelity of report	.	.	.	.	.	.	.
7. Line lengths	·16 (·085)	.	–·03 (·087)	.	·25 (·081)	.	.

3. *Comparison of the results of the tests with teachers' estimates.*

Separate estimates were made by two or more of their teachers with respect to certain qualities of the pupils who acted as subjects in these tests; and correlations were ascertained between these estimates. For school *B*, *r* between different teachers' estimates for the same children for G (General Intelligence) = ·49. If we pool tests Nos. 1 to 6 for this school in two groups (Nos. 1, 3 and 5, and Nos. 2, 4 and 6, together), we can obtain a coefficient of correlation between them. This works out at *r* = ·48 or (including test No. 7 in the odd-numbered tests) *r* = ·44. What is the meaning of this figure? It signifies that the estimates of suggestibility, as measured by one group of tests, are related to those measured by the other group; and it shows a common factor or factors to have been operating in all the tests. The two groups of tests can be likened to two sets of examination papers by which the general information or ability of those sitting for examination is estimated, or to the estimates of the two or more teachers as to the capability of their pupils under any given head. As a matter of fact, the closeness of the two *r*'s in this case indicates that either group of the tests, as a measure of the suggestibility of the subjects, may be taken as an estimate of it about as good as the formed opinion of any one of their teachers upon the qualities which they evaluated.

Similarly, correlating test No. 1 with the pool of tests Nos. 3 and 5 (school *A*) we get *r* = ·29; or taking the pool of tests Nos. 1 and 3 with that of tests Nos. 5 and 7, *r* = ·21. The estimates of the two teachers for the subjects of these tests under the head of quickness of apprehension was, *r* = ·71. The large discrepancy in this case is probably due to a very considerable extent, at least, to the fact that the estimates were made by the form master and the head master of the school; a pair of estimates which as a rule are not so satisfactory as those of independent form masters.

Correlations were also ascertained between suggestibility (S) as evidenced in the tests and certain estimates of character qualities made by teachers in school *A*. The only significant ones, for our purpose, seemed to be:

Tendency to abandon tasks in face of obstacles and S; *r* = – ·36.
Soundness of common sense and S; *r* = ·75.
Originality of ideas and S; *r* = ·21.

The following correlations of S with other tests are of interest. The same group of subjects (school *B*) was tested for Perseveration (P) by

Dr Wynn Jones, and for General Intelligence (G) by Mr Burt, for Oscillation (O)[1] by Mr Flügel, and for Motor Dexterity (M) by Miss Carey. Pooling the G, P, M and O tests severally and the first six tests of S, we get

S and G; $r = \cdot 15$.
S and P; $r = -\cdot 08$.
S and M; $r = \cdot 10$.
S and O; $r = \cdot 04$[2].

There appears to be no tendency, positive or negative, for S to accompany G, P, M or O.

Prestige and its absence. If we compare the tests 'with prestige' (Nos. 1, 4, 6 and 7) with those 'without prestige' (Nos. 2, 3 and 5) by severally pooling and correlating them, we find: S with, and S without prestige; $r = \cdot 40$. But we have reason to think that this classification of prestige and non-prestige tests is faulty; and that the real prestige tests in these experiments were Nos. 1, 3 and 4. (See Part III.)

II.

During the Summer Term of 1920, the second of the writers of this paper received the results just given (schools *A* and *B*) and carried out a further series of tests on 56 children (33 boys, average age 13·2, and 23 girls, average age 13·6) in another school (*C*).

With one exception (test 6; Fidelity of Report), the tests used were the same as those already described. The methods of carrying out the tests and of marking were also the same as those used in the first research.

1. *Frequency distributions for the various tests.*

The following results were obtained[3].

Test 1. Hand Rigidity by Suggestion.

The test was given twice, once with each hand, and the reliability right hand *vs.* left hand worked out $r = \cdot 99$ for boys and ·94 for girls. Grading as before, but not dividing grade 1 into two grades on a time basis, the frequency distribution for classes 1 to 8 is: 18, 2, 3, 1, 6, 7, 9, 10.

Dividing the subjects into (i) those who could, and (ii) those who could not, close their hand, 73 per cent. Boys, 35 per cent. Girls, Total 57 per cent., could close their hand, while 27 per cent. Boys, 65 per cent. Girls, Total 43 per cent., could not.

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[1] Of attention, as in reversible perspective, etc.

[2] N.B. One subject was omitted in the O tests.

[3] In order to economise space the Boys' and Girls' results, calculated separately, are here pooled, and the distributions given in the briefest possible form.

Test 2. Suggestion by Progressive Lines.

The frequency distribution for this test is shown in

Table 12.

Marks	15	.	14	.	13	.	12	.	11	.	10	.	9	.	8	.	7	.
Subjects	.	.	.	.	.	.	.	1	.	.	.	1	1	2	.	3	1	2

Marks	6	.	5	.	4	.	3	.	2	.	1	.	0	.	−1	.	−2
Subjects	3	.	8	8	3	5	8	1	5	1	.	.	1	.	2	.	.

Test 3. Suggestion by Illusion of Warmth.

The test was performed as before, ten times with each subject. The reliability for right hand *vs.* left hand was: Boys, $r = \cdot 99$; Girls, $r = \cdot 99$. The frequency distribution (both hands pooled and averaged, and set out in 5″ intervals) is given in the following table:

Table 13.

Time (secs.)	0–5	5–10	10–15	15–20	20–25	25–30	30–35
Subjects	11	5	4	1	3	3	1

Time (secs.)	35–40	40–45	45–50	50–55	55–60	60
Subjects	5	4	1	1	1	16

Test 4. Hand Levitation by Suggestion.

The test was performed twice with each subject, once sitting and once standing. The reliability between the two was: Boys, $r = \cdot 81$; Girls, $r = \cdot 93$. The results for the two hands were then pooled and averaged, and the following frequency distribution obtained for classes 1 to 9: 16, 2, 3, 4, 0, 0, 2, 6, 23.

When the subjects were divided into those whose hands rose from the table, and those whose hands did not, it was found that 24 per cent. Boys, 74 per cent. Girls, Total 44 per cent., belonged to the former, and 76 per cent. Boys, 26 per cent. Girls, Total 56 per cent., belonged to the latter class.

Test 5. Suggestion by Progressive Weights.

The same three methods were used as before. The reliability coefficients between them were: Boys and Girls pooled, Method 1 with Method 2, $r = \cdot 35$; 1 with 3, $r = \cdot 56$; 2 with 3, $r = \cdot 55$. The lower reliability in school *C* compared with *A* and *B* may be due in part to the large number of errors in judging the first four weights where there was actual increase. One or more errors were made in method 1 by 42 per cent. Boys and 39 per cent. Girls; and in method 2 by 18 per cent. Boys and 22 per cent. Girls. There were no errors in method 3. Pooling the marks for the three methods the frequency distribution (marks 30 to 0 distributed in fives) is: 1, 6, 8, 15, 19, 7.

Test 6. Fidelity of Report.

This test was only given to the Girls, two of whom were absent; the numbers being thus reduced to 21. The method of administering it differed greatly from that used in Part I. There the questions were asked individually and immediately after the pictures had been seen. In the present instance the subjects had written a description of two pictures (the two given by Terman in the Test Material for use with his *Measurement of Intelligence*) a month previously, in connexion with another research. A questionnaire for each of the two pictures was drawn up; and in it were included questions of a nature similar to those described in Part I. The questionnaire was read to the subjects as a group, and the answers to the questions written down as each was read out. The method of marking used in Part I was adopted, and the reliability coefficient between the two pictures worked out: $r = \cdot 62$. The marks for the two pictures were then pooled and a frequency distribution made. This proved so similar in form to the one obtained in the former research that it is included here. The total of possible marks was 41. The results (marks 40 to 0 distributed in fives) are: 0, 2, 0, 3, 3, 3, 6, 3.

Test 7. Suggestion of Line Lengths by Personal Influence.

In the majority of cases the suggestion seemed to arouse contrary suggestion in the subjects, especially in the case of the Boys, while those who yielded appeared to do so with reluctance. The frequency distribution for marks 6 to 0 was: 0, 0, 0, 2, 2, 6, 46. Owing to these poor results this test was omitted when working out the correlations between the tests.

2. *Correlations between the tests.*

These were worked out between the various tests by the method of ranks and the formula $r = 1 - \frac{6\Sigma d^2}{n(n^2-1)}$. They are shown (together with their probable errors) in

Table 14.

Boys—Girls (pooled and weighted).

	1	2	3	4	5
1. Hand rigidity	.	·17	·06	·73	·28
2. Progressive lines	·17 (·091)	.	− ·24	·15	·24
3. Warmth illusion	·06 (·094)	− ·24 (·088)	.	·23	·09
4. Hand levitation	·73 (·043)	·15 (·091)	·23 (·088)	.	·24
5. Progressive weights	·28 (·086)	·24 (·088)	·09 (·094)	·24 (·088)	.

The correlation between the odd numbered tests (1, 3, 5) and the even numbered ones (2 and 4) was also worked out (see Part I, p. 61): $r = \cdot 318$.

3. *Correlation of suggestibility with other factors.*

The subjects were also tested for General Intelligence (G); and Perseveration (P) (by Dr Wynn Jones' tests) and compared with their ranking for Suggestibility. Tests for O and M were not performed. The final order in each case was calculated by pooling the ranking in the individual tests (in the case of Suggestibility, Nos. 1–5). Results: S and G, $r = \cdot 01$; S and P, $r = \cdot 00$.

The correlations between Suggestibility and the following character qualities, as estimated by the class teacher, are: S and Common Sense—Girls, $r = -\cdot 13$; Boys, $r = \cdot 29$; Pooled, $r = \cdot 12$. S and Originality of Ideas—Girls, $r = -\cdot 45$; Boys, $r = -\cdot 03$; Pooled, $r = -\cdot 20$. S and Tendency to Abandon Tasks in Face of Obstacles—Girls, $r = \cdot 48$; Boys, $r = -\cdot 09$; Pooled, $r = \cdot 14$.

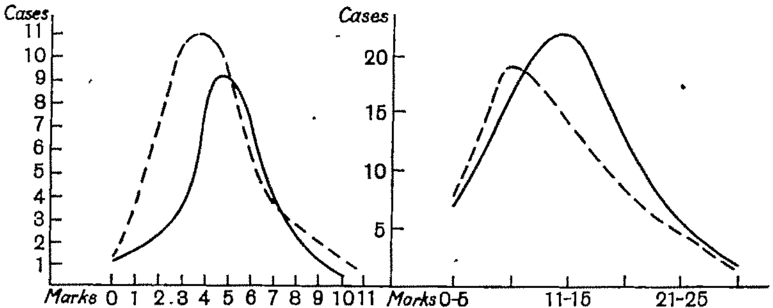
Common Sense as estimated by a teacher is doubtless a quality largely consisting of affective and conative elements, and not purely cognitive. The cognitive aspect may be indicated by two additional tests given to the subjects—namely two "Probable Situations" tests, similar to those described by Dr Webb in *Character and Intelligence*: "If such and such a thing happened, write down (i) all the things you *could* do; (ii) say which of them you *would* do; (iii) say *why*." The reliability between these two tests (marks for sections 1, 2, 3 pooled) was: Boys, $r = \cdot 70$; Girls, $r = \cdot 80$. The marks for the two tests were then added together and a fresh ranking made. The correlations of S with Common Sense, as so measured, are: Boys, $r = \cdot 00$; Girls, $r = -\cdot 45$; Pooled, $r = -\cdot 18$.

III.

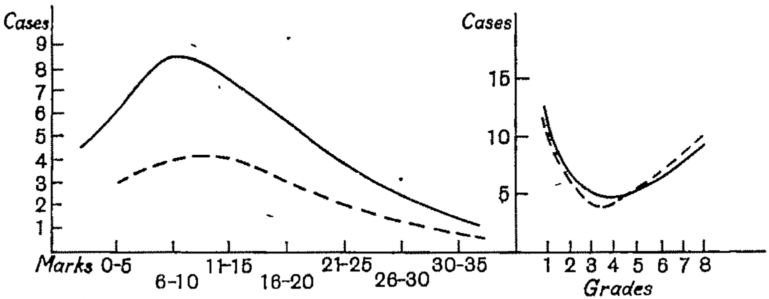
1. *Comparison of the frequency distributions reached in the two researches.*

The first significant feature is the correspondence of the frequency distributions for the same tests in the two researches. It will have been noticed already that in the separate researches there was a marked correspondence between the form of distribution for Boys and Girls. This is also found when the results obtained by two different experimenters, on children differing in age by 1·5 years and living in different surroundings, are compared.

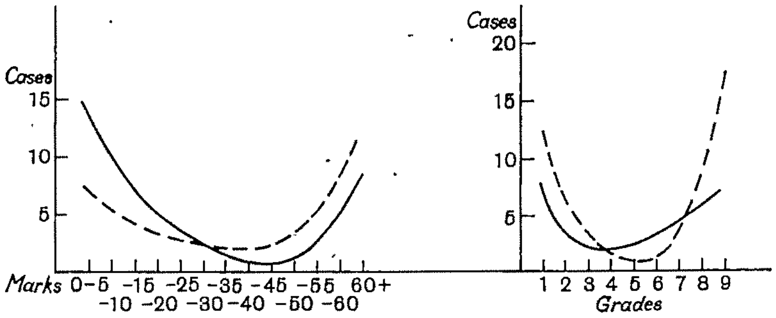
Reference to the tables given will show this; but it is made more evident by exhibiting the results graphically, as is done below. Indicating the results (Boys and Girls pooled) of the first research by ———, and of the second by - - - - - - -, the following 'smoothed' curves are obtained mathematically[1].



Test 2. Progressive Lines. Test 5. Progressive Weights.



Test 6. Fidelity of Report. Test 1. Hand Rigidity.



Test 3. Warmth Illusion. Test 4. Hand Levitation.

Fig. 1.

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[1] A final slight smoothing was made by eye.

For the same test the form of curve is identical. Taking the tests as a whole, the curves form two distinct types (i) an ordinary, more or less, symmetrical curve, and (ii) a 'U' curve.

The tests producing the latter are the two Hand tests with Personal suggestion, and the Warmth Illusion. The first two are obviously tests in which the factor of prestige, or personal suggestion, is at a maximum; and in the warmth test the same factor, though not so evident, is weighty. Not only is the imposing character of the electrical warmth-tester a strong cause of the suggestion of heat; but the personal attitude of the experimenter seems to be even a stronger one. His manifest general attitude towards the apparatus and the subject of the experiment, his imposing expectancy, his manipulation of the visible switch, all go to form an amount of prestige which is lacking in other tests formerly regarded as prestige tests, such as that based on the Fidelity of Report test. The "Suggestion of Line Lengths by Personal Influence" may be a prestige test; but if so, at any rate in the present research, it has operated mainly in an inverse manner, and, if anything, produced contra-suggestion.

If the form of curve is to be taken as an indication, then these three tests (Hand Rigidity, Hand Levitation and Warmth Illusion) should be classed as the prestige tests[1].

With regard to the 'Report' test, the correspondence of the curves is remarkable, considering the different conditions in which the experiments were conducted. Though it seems natural to regard this test as mainly a prestige test, the form of curve and the fact that it would probably be equally effective with a printed form of questions and instructions appear to indicate that prestige is not a large factor in it.

What is the significance of these two divergent types of distribution?

Can we say that, in the case of suggestion by prestige or personal influence, subjects tend to be either very suggestible or very little so; and that in the case of suggestion with a minimum of prestige, or personal influence, suggestibility tends to have a normal distribution? And if so, are there two distinct types of suggestibility? Or are the different curves due to the nature of the tests given and the method of marking?

From an inspection of the curves the latter seems more probable. In all tests of suggestibility account must be taken of three modes of response—positive, neutral and negative[2], the latter being contra-suggestion. In any given test the distribution may measure any one, two or three of these factors.

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[1] If in the warmth test the subjects are graded, not by time, but in a scale from 1 to 10, according to the number of failures of suggestion recorded, the curve obtained is still the same.

[2] Prideaux, this *Journal*, x. 235 *sq.*

Theoretically then, careful measurement of the subjects' total reactions might produce such a distribution as follows in which the three modes of response are all indicated.

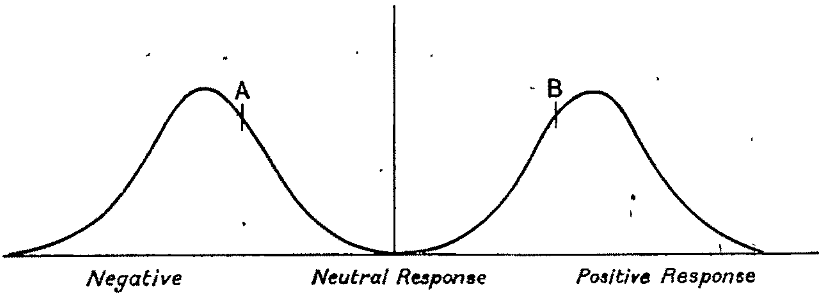

Fig. 2.

Practically the distributions overlap, owing to the difficulty of distinguishing neutral and negative responses.

Now compare the tests. In those with prestige emphasized there is (and this fact is confirmed by observation of the subjects) a much greater liability that contra-suggestion will be aroused. The conditions of the experiment and the method of marking make it impossible to distinguish between neutral and negative responses; so, to a small number of neutral responses, all the negative responses are added, producing a large number of cases at one end of the scale. By the method of marking we also more or less fail to evaluate accurately and to distinguish clearly cases of great and moderate suggestibility. Consequently a large number of cases is also grouped at the other end, and the curve takes the form of a U. In the curve given, what happens is that all the cases of positive responses greater than *B*, and of negative greater than *A* are summed up; and the resultant curve is the portion between *A* and *B* with the parts beyond added towards the two extremes.

In the non-prestige tests the factor of contra-suggestibility is absent, or only slightly present. Also here the marking admits of differences between moderate and extreme suggestibility being noted. Therefore the distribution *tends* towards a normal one. Possibly the skew in the curve is due to a certain number of cases of contra-suggestion. This would yield lower marks and thus account for the steeper side of the curve.

It is suggested that the test (No. 7) which was mostly a failure can be explained on the same assumption. We have in this case only to regard it as a test of contra-suggestion. Then all failures to take the given suggestion are measured in terms of contra-suggestion[1]; and the method of marking allows no recognition of degrees. What is obtained is practically one-half of a normal curve, with all the cases belonging to the second half summed up, *i.e.* distribution curve for pooled results of test No. 7.

In this test, if a system of grading could be devised to mark prompt and decided refusal of the suggestion and the degrees of hesitation, etc., which lead on gradually to acceptance, then satisfactory results should be obtained; and possibly a distribution approximating to that in Fig. 2 would result.

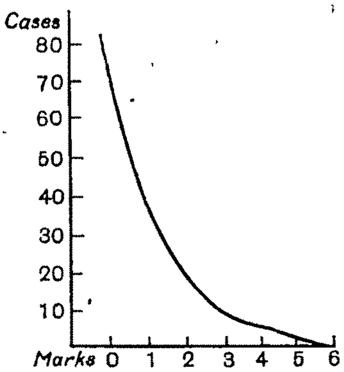

Fig. 3.

There seem then no grounds for thinking that 'personal' and 'impersonal' suggestibility are of two distinct types. The results obtained from 'personal' and 'impersonal' suggestion will however be different. In the former directly positive or negative responses may be expected; and these are found to be about equal in number. In the latter there will be very few failures and the majority of subjects will be moderately suggestible. This result has a distinct bearing on many educational problems.

2. *Discrepancies in the results.*

Certain discrepancies in the results are interesting. In the Hand Rigidity and Hand Levitation tests the percentage of boys influenced as compared with that of girls is different for the two experimenters. In the second research the results obtained from the boys were low, and from the girls high. One significant cause seems to be that during the war the boys' class had no male teacher, and their discipline consequently deteriorated. Another was that the experimenter was known to most of them, and lacked the additional prestige surrounding a complete stranger. The girls' class however had been in charge of a capable teacher of long standing in the school, and was under complete control and discipline.

Another interesting difference comes out in comparing the Hand Rigidity and Hand Levitation tests. In both researches the latter was comparatively more effective in inducing suggestibility with the girls, and the former with the boys. If as maintained by Dr Prideaux (in the article referred to) suggestibility "depends on the affective processes operating between the two persons," and that the sexual instinct is a distinct factor, then the difference noticed above might be accounted for by the additional effect of personal contact on the girls. In connexion with this and with the psycho-analytic view that suggestion is a special variety of transference, and takes its root in "the masochistic component of the sexual instinct[1]," it is interesting to observe that in both these tests there is no significant correlation with the age of the subjects of this research.

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[1] No doubt many may be included of the kind described above as neutral.

3. *Comparison and summation of the correlations.*

(*a*) Comparing the two researches, considerable fluctuation in the correlations is observed; and there are some the magnitude of which does not exceed three times the P.E., or is less than the P.E.; but in general, though small, they are positive in all the schools. Two discrepancies stand out: the very much larger correlation obtained between the Hand Rigidity and Hand Levitation tests in the second research; and the larger one between Hand Rigidity and Warmth Illusion in the first. This latter difference may well be due to the varying amount of prestige two different experimenters might introduce into these tests. Another difference is that the correlation between groups of tests is smaller in school *C*. Also the correlations between S and character qualities are different: there being a very small correlation with Common Sense in school *C* (·12) instead of a large one in school *A* (·75); one of − ·20 with Originality of Ideas instead of ·21; and ·14 with Tendency to Abandon Tasks in Face of Obstacles instead of − ·36. But since in school *A* only boys were tested we may separate the results yielded by boys and girls in school *C* for purposes of comparison, as these differed considerably. The results (boys only) are:

	School *A*	School *C*
S and common sense	$r =$ ·75	$r =$ ·29
S and originality of ideas	$r =$ ·21	$r = -$·03
S and tendency to abandon tasks	$r = -$·36	$r = -$·09

(*b*) Pooling (and weighting) the correlations for all three schools, we obtain the following table. The total number of subjects for tests Nos. 1, 3 and 5 is 121; for tests Nos. 2 and 4, 94; for test 6, 59. (P.E. is shown in brackets.)

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[1] Prideaux; *op. cit.*

Table 15.

	1	2	3	4	5	6
1. Hand rigidity	.	·27	·30	·55	·28	·16
2. Progressive lines	·27 (·071)	.	– ·01	·18	·27	·32
3. Warmth illusion	·30 (·058)	– ·01 (·077)	.	·20	·11	– ·25
4. Hand levitation	·55 (·053)	·18 (·074)	·20 (·074)	.	·17	– ·06
5. Progressive weights	·28 (·059)	·27 (·071)	·11 (·062)	·17 (·075)	.	·00
6. Fidelity of report	·16 (·089)	·32 (·082)	– ·25 (·085)	– ·06 (·092)	·00 (·092)	.

The correlations between the columns of correlations were also worked out to see if the conditions for a hierarchy obtained. The results are:

Table 16[1].

	1	3	4	2	5	6
1. Hand rigidity	.	·81	·99	– ·31	·42	·05
3. Warmth illusion	·81	.	·93	– ·64	·77	– ·45
4. Hand levitation	·99	·93	.	– ·11	·81	·24
2. Progressive lines	– ·31	– ·64	– ·11	.	– ·01	·92
5. Progressive weights	·42	·77	·81	– ·01	.	·96
6. Fidelity of report	·05	– ·45	·24	·92	·96	.

These, when corrected by the Hart and Spearman formula[2] become

Table 17.

	1	3	4	2	5	6
1. Hand rigidity	.	·94	[·99]	[– ·31]	·62	·06
3. Warmth illusion	·94	.	·99	[– ·64]	·88	– ·46
4. Hand levitation	[·99]	·99	.	– ·25	·94	·24
2. Progressive lines	[– ·31]	[– ·64]	– ·25	.	– ·01	1·09
5. Progressive weights	·62	·88	·94	– ·01	.	[·96]
6. Fidelity of report	·06	– ·46	·24	1·09	[·96]	.

[Coefficients in brackets are those from columns to which the formula could not be applied.]

What does this signify?

Dividing the table so as to mark off the Prestige and Non-prestige tests, it will be noted that the corrected correlations[3] between the former are all high and positive approaching + 1 (av. = ·97). Of the three correlations between the Non-prestige tests, two are practically + 1, the other being approximately 0. The correlations between the two groups vary from + ·94 to − ·64. Those between a Prestige test and tests 2 and 6 being low and in general negative (av. = − ·22), between a Prestige test and test 5 high and positive (av. = + ·78). These facts seem to suggest that the correlations are produced neither by a single general factor, nor by a number of specific factors, nor by two group factors such as Prestige and Non-prestige; but by a combination of a general factor with group and specific factors.

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[1] In this and following tables, the tests are printed in a different order from that previously used; but they are numbered as before.

[2] This *Journal*, 1912, 58.

[3] The conclusions that follow can also be drawn from the uncorrected correlations, and are therefore independent of the validity of the Hart and Spearman formula which has been questioned. This *Journal*, x. 320.

4. *The problem of a 'general factor.'*

It was noticed during the experiments that in a number of cases certain subjects proved very suggestible to all the tests, or nearly all; while others accepted practically no suggestions. This would point to a general factor of suggestibility strongly marked in some subjects. It has also been pointed out that contra-suggestibility was a factor entering especially into the Prestige tests. This may then be regarded as a group factor influencing them. The similarity of method in tests 2 and 5 may also make certain elements operate as a group factor in these two tests.

Assuming the existence of a general factor, and of these group factors, the values obtained in the table of correlations between columns could be partly explained. The effect of the group factors will not affect, to any appreciable extent, the correlations between the Prestige tests (1, 3 and 5) which will be approximately + 1, nor those between the Non-prestige tests which should also approximate to + 1; while those between a Prestige test and tests 2 and 5 will be slightly reduced. This obtains with certain exceptions: namely the greater reduction between the Prestige tests and tests 2 and 6, and the low correlation between tests 2 and 5.

These discrepancies would be explained if there was a largish common element, in addition to Suggestibility, operating as a group factor in tests 2 and 6. Analysis of the tests and methods of procedure has, however, not revealed such a factor. Still it may exist, or possibly experimental errors, augmented by the lower reliability of all correlations with test 6 (due to the fewer number of cases), may have produced spurious correlation. In any case further research seems necessary.

The authors consider that, taking this into consideration, the most probable explanation of their results is that advocated, namely, the existence of a general factor of Suggestibility combined with group factors common to two or more tests.

The following correlations between groups of tests seem to support this opinion. The weighted average of all the correlations between the

Prestige tests (Nos. 1, 3 and 4) = ·34; between the Non-prestige tests (2, 5 and 6) = ·21; between tests 1, 3 and 5 = ·23; between tests 2, 4 and 6 = ·15. The weighted average of all the correlations between each Prestige and each Non-prestige test = ·10; and between tests 1, 3 and 5 and 2, 4 and 6 = ·16.

Assuming the existence of a general factor the tests were examined from the point of view of their Saturation of Suggestibility[1] by the formula $r^2as = \frac{A^2 - A'}{2T - 2A}$; where r^2as is the square of the correlation between any test (a) and Suggestibility; A is the sum of the correlations between this test (a) and every other test; A' is the sum of the squares of these correlations; and T is the sum of all correlations between the different tests. The calculation gives the following results:

Saturation of pooled tests (omitting tests 6 and 7)

r (test 1 and S) = ·88,
r (test 2 and S) = ·31,
r (test 3 and S) = ·26,
r (test 4 and S) = ·61,
r (test 5 and S) = ·40.

The pooled correlations for S and G = ·06; S and P = ·03.

There is no tendency shown for Suggestibility to correlate with General Intelligence, Perseveration, Oscillation or Motor Dexterity. With regard to Common Sense, it is to be observed that the correlation between this (as estimated by teachers) and S is positive in the case of the Boys (school A, r = ·75; school C, r = ·29; Pooled, r = ·44). In the case of the Girls of school C it is negative (r = − ·13). The correlation with Common Sense, as measured by the "Probable Situations" tests and S is Boys, r = ·00; Girls, r = − ·45.

Comparing these figures, it is interesting to note that, however measured, the Boys show a higher correlation between Common Sense and S than the Girls. Also it has already been suggested that affective and conative as well as cognitional elements enter into the data on which the teachers' estimates are based, and that these are absent in the "Probable Situations" tests. There is no room in these for contra-suggestion; and this may be a weighty factor in the case of the estimates. The contra-suggestible child may well appear stupid at times, and be classed as low in Common Sense as well as in Suggestibility.

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[1] See "Mental Tests of Dementia," by B. Hart and C. Spearman, *Journ. of Abnor. Psy.* Oct.–Nov. 1914.

IV. SUMMARY.

1. Suggestion arises out of the total environment and conditions to which a subject is exposed. When personal prestige is the most conspicuous element in a test or situation, the suggestion arising may be called a 'Personal' or 'Prestige' suggestion. When this is at a minimum and the conditions the main factor, the suggestion may be called an 'Impersonal' or 'Non-prestige' suggestion.

2. In cases of 'Personal' suggestion a negative response, owing to the development of contra-suggestion, is frequent. Consequently subjects tend to be classified into two sharply divided groups, the suggestible and the non-suggestible (or contra-suggestible).

3. In cases of 'Impersonal' suggestion, as obtained in our tests, contra-suggestibility is not so frequently aroused, and the distribution is approximately normal, the majority of the subjects being moderately suggestible.

4. There is evidence which points to a general factor of Suggestibility complicated by group factors.

5. General Suggestibility is greatly modified by the specific conditions and elements of the whole situation, which vary in individual cases, according to experience of it and knowledge about it.

6. There is no ascertained tendency for Suggestibility to go with other 'general' factors such as General Intelligence, Perseveration, Oscillation, or Motor Dexterity. There is a small correlation between it and Common Sense regarded as consisting largely of affective and conative elements; but none if Common Sense is considered as a purely cognitive quality.

(*Manuscript received* 1 *January* 1921.)

RECENT WORK IN EXPERIMENTAL AESTHETICS.

By EDWARD BULLOUGH.

To speak at the present moment of 'recent' work in experimental Aesthetics is perhaps a little misleading. No work of an experimental kind has, to my knowledge, been carried out since 1914, and what is meant here by 'recent' are experiments undertaken roughly between 1900 and 1914. It is, however, precisely the lack of experimental work during the last six years which prompts me to attempt a cursory survey of some of the earlier results. For now is the time to take stock of what has been done so that we may build as best we can upon what has been achieved, and survey the unexplored regions lying before us for investigation. This is one of the main objects of this paper.

Its other object is to direct attention to certain problems rather than to others, problems, the examination of which seems to me to promise specially useful results, because the problems themselves appear to me to lie on the main road followed of recent years by aesthetic speculation.

In order to explain, I may perhaps be allowed very shortly to touch upon the earlier beginnings of experimental work in Aesthetics. They are associated with the name of the man whom every student of the subject knows as 'the father of modern Aesthetics' and also as the originator of experimental work in it—Theodor Fechner. In his *Vorschule der Aesthetik*, published in 1867, he gave among other results of his thought the results of his experimental examinations which, by a peculiar irony of fate, had been directed originally to testing the validity of the most rigidly formalistic doctrine of his day, Zeising's 'golden section.' It would be waste of time to recapitulate Fechner's experiments or results. They have been summarised repeatedly, as for instance in C. Lalo's *Esthétique expérimentale contemporaine* (1908). They have in addition been tested all over again, not without interesting results, in a special set of experiments[1] carried out in America and have led within the period here under discussion to a considerable body of work dealing especially with spatial forms such as the divisions and arrangements of lines, balance and symmetry, etc. I do not propose to deal with these experiments here except in so far as they bear upon the problems to which I should like to draw attention. But I take this opportunity to say that there is hardly an experiment carried out in the past which it is not worth while testing again, in order to interpret it in the light of the constantly developing theories of aesthetic experience.

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[1] L. Martin: "An experimental study of Fechner's Principles of Aesthetics," *Psychol. Rev.* XIII. 142 ff.

Fechner's experiments cannot be said to have yielded any very striking or illuminating positive results. The reasons of their failure are well known and are set out in every criticism, such as can be found in the above-mentioned work of Lalo. The purely quantitative measurements, and the system of averages which he employed, were the direct consequences of the hedonistic principles on which he based his theory of Aesthetics in general. The search for the colour which the greatest number of persons prefer, the questions whether squares or oblongs are more generally liked, or what proportions of the arms of a cross meet with the widest approval, seem to us rather crude and naïve. But posterity owes a great debt of gratitude to Fechner for having attacked these kinds of problems, even if the results showed that little progress could be hoped for in that direction.

Now that we can look back upon nearly fifty years of aesthetic speculation "from below," as Fechner said, it is of interest to note that experimental work has followed very closely the trend and evolution of aesthetic theory in general. It would seem as if aesthetic speculation, in the attempt at a better and closer understanding of the aesthetic experience, had proceeded to analyse this experience into its component stages in the reverse sequence to that in which they might be supposed to follow and develop out of each other, and had gradually worked its way up stream, so to speak, from pleasure, the point at which the experience issues out into the sea of practical life, to the still hidden sources of its obscure beginnings.

Fechner will always be remembered in Aesthetics for having opened up Association as a possible aesthetic factor and for having thereby rescued Aesthetics from the narrow formalism into which it had fallen in his day. The introduction of Association led, partly in conjunction with the romantic views of the artist and his functions, to the theory of Expression and of the apprehension of this Expression as the central fact of the aesthetic experience. Its psychological examination produced the various theories of *Einfühlung* or Empathy which reached their flowering period between 1895 and 1905. Association has been much experimented upon, of course, as a fact of general psychology; but from the purely and specifically aesthetic point of view experimental work on

Association as a factor in the aesthetic experience has always presented considerable difficulties, partly by reason of its erratic and uncertain occurrence. Nor is there any doubt that its importance has been greatly exaggerated in the past and is at the present time grossly overrated in popular opinion.—Empathy and all the numerous *Einfühlungs*-theories were exclusively based upon introspection on the part of their authors. Indeed, the great varieties of views of their mechanism and the acrimonious wrangles which took place at the end of last century between the upholders of rival doctrines arose precisely from the generalisation of such purely personal introspective evidence. Experimental work on large numbers would, I believe, have shown that no single one of the explanations championed by different adherents of the theory could claim the monopoly of truth. In justice it must, however, be added that experimental tests of Empathy are extremely difficult to carry out. The relatively systematic observations of Vernon Lee and A. Thompson[1] are very suggestive but, being introspections of the authors themselves, have probably accentuated the idiosyncracies of the experiences. Many of the experiments designed to investigate Empathy either failed altogether, or brought to light points relating not to it, but to the matters with which I propose to deal in the sequel.

Meanwhile aesthetic theory did not stop at Empathy. Formalists and others hastened to point out that Empathy was unable to take satisfactorily into account the incontestable aesthetic qualities of purely formal arrangements, of repetition and symmetry, of composition and finish of workmanship. The adherents of *Einfühlung* tried to meet this attack, as Lipps, for instance, did in his brilliant and suggestive, but not wholly convincing *Raumaesthetik*[2]. The fact, however, remains that Empathy does not invariably occur in aesthetic appreciation. There proved to be an even more insidious line of attack, for it was found that Empathy occurs occasionally with exceptional clearness in what is admittedly bad art, or not an object of aesthetic appreciation at all. In any case it is fairly clear that Empathy may occur in aesthetic experience, but that it need not, and that it is neither a complete nor wholly satisfactory explanation of it.

The discussions of Empathy and the examination of the conditions under which it occurred had, however, the great merit of leading to attention being directed to what might be called the 'pre-empathic' stages of aesthetic experience. It was in response to the questions, how does Empathy come about, what makes Empathy possible, that these stages rose into prominence. For was it not reasonable to suppose, if Empathy occurred both within and also outside aesthetic experience, that that which in the one case made it into part of the aesthetic experience was something that preceded it and gave it its aesthetic tinge? And was it not equally reasonable to assume that the same thing happened with all the other ingredients of the aesthetic experience, with emotions and associations and pleasure and judgments? If this were really the case, would we not then have discovered in these pre-empathic stages the real *differentia* of the aesthetic from all other kinds of experience?

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[1] Vernon Lee and A. Thompson: "Beauty and Ugliness," *Contemp. Rev.* 1904; reprinted in Vernon Lee and A. Thompson's *Beauty and Ugliness* (1912, John Lane).

[2] Th. Lipps: *Raumaesthetik und geometrisch-optische Täuschungen*, 1897.

These are the problems upon which I venture here to direct attention. Experimental work upon them has been very sporadic. Much of the earlier work has unconsciously touched upon them, most frequently ignoring them, but sometimes throwing out valuable hints. At any rate we cannot be said to know much about them and for that reason they appear to me more worthy of attack and of a sustained effort to bring them to the test of actual experiment.

These pre-empathic stages of aesthetic experience comprise, in the first place, *the apprehension* of the aesthetic object, and secondly, antecedent to it, *the adaptation* initiating the apprehension.

I shall deal with the results of experimental work on the apprehension of the object first.

APPREHENSION.

The study of the apprehension or apperception of objects is a question of General Psychology rather than of Aesthetics, and a great deal of the experimental work done upon it by General Psychology has a direct bearing on the aesthetic problem. Experiments have, however, been tried also from a specifically aesthetic point of view in this connexion, and one of the important means of research has been brought within the scope of aesthetic experiment by the introduction of tests with short-time exposures. It is interesting that this was attempted for the first time in an endeavour to deal with Empathy. Külpe[1] tried to 'cut out' Empathy by offering to subjects a series of slides of architectural, sculptural and pictorial works with a short exposure of 3 seconds. The experiment failed, for even 3 seconds were sufficient to allow empathic states to emerge and associations to be formed. I had the same experience with single colours where even a fraction of a second was insufficient to cut out Empathy and Association[1]. But, besides directing attention to the time factor in the development of the aesthetic state—a point so far very inadequately tested—it gave the negative result that Fechner's 'direct' factor, which these experiments had hoped to isolate, was so intimately bound up with what he supposed to be the 'indirect' (associative) factor as to be practically inseparable. This and the discovery of adaptation raised in a concrete form the problem whether the apprehension of aesthetic objects could be treated as identical with that of non-aesthetic objects, in other words, whether there was such a thing as a definitely *aesthetic* apperception. Speculative Aesthetics had among many other things always tacitly assumed that there was, and the importance of the point is evident. For if there is anything aesthetic in the apprehension itself, it is conceivably this quality of the apprehension which in the first instance imparts the peculiarly aesthetic character to all the subsequent stages of the experience.

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[1] O. Külpe: "Ein Beitrag zur experimentellen Aesthetik," *Amer. J. of Psychol.* 1903, XIV. 215 ff.

Unfortunately there is a great dearth of accurate, experimentally ascertained knowledge on this question. Very little work has been done as yet from the specialised point of view of such a problem. The little information available is derived almost exclusively from experiments in the apprehension of pictorial objects and simple spatial forms and colours. A fair amount of research has indeed been undertaken with temporal forms of a simple kind, especially with rhythm[2], and the first really useful experiments on the apperception of single tones and simple tone-combinations are now available. But the apperceptive processes in the appreciation of poems or dramatic works are, except from a few stray observations[3], so far quite unknown.

Even in the case of spatial forms, our information is derived not so much from aesthetic experiments yielding introspective evidence of the subjects[1], as from tests demanding a written description of a previously apperceived object. Such descriptions—as distinct from the actual apperception—give certainly indications of the manner of the apprehension which they reflect, but are open to generally admitted objections. They usually omit to state all the apprehended details and involve, as written statements, a transfer of visual impressions into language[2]. They require especially as literary productions, however humble, a productive effort, alien to the merely apprehending process and to some extent destructive of appreciation[3]. The former drawback attaches, of course, also to introspective evidence, but is much more easily corrected in the spoken intercourse between experimenter and subject.

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[1] Edward Bullough: "The 'perceptive Problem' in the aesthetic Appreciation of single Colours," this *Journal*, II. 406.

[2] E. Meumann: "Untersuchungen zur Psychologie und Aesthetik des Rhythmus," *Philos. Stud.* x. 249; M. Ettlinger: "Zur Grundlegung einer Aesthetik des Rhythmus," *Ztsch. f. Psychol.* 1900, XXII. 161; R. McDougall: "The Structure of simple Rhythm-forms," *Harvard Psychol. Stud.* 1903, I; R. H. Stetson: "Rhythm and Rhyme," *Harvard Psychol. Stud.* 1903, I: "A Motor Theory of Rhythm and discreet succession," *Psychol. Rev.* 1905, XII.; K. Koffka: "Experimental-Untersuchungen zur Lehre vom Rhythmus," *Ztsch. f. Physiol. d. Sinnesorg.* LII. 1.

[3] M. Dessoir: "Anschauung und Beschreibung," *Arch. f. syst. Philos.* N.F. 1904, X. 20 ff.

In the absence of specifically aesthetic experiments, it is impossible to state any definite conclusions. But the available material of experimental work undertaken from the general psychological standpoint suggests at least certain problems the solution of which would materially advance the question of an 'aesthetic' apperception.

Binet[4] distinguishes in his *Experimental Study of Intelligence* four types of descriptions corresponding to four different types of apprehension: The *type descripteur*, the *type observateur*, *type d'érudit* and *type imaginatif* or *émotionel*. Members of the first type content themselves with a pure and bald description of the details of the object, without even seeking to grasp their significance, much less their relation or the meaning of their collocation. This type yields, as Binet expresses it, a mere "*description de nature morte.*" The second type 'observes,' *i.e.* attempts to seize and gauge the expressive and significant features of an object; it realises that not all the details are of the same importance, but bear a definite relationship of significative values both to each other and to the whole. This type gives, in his words, "*une description de tableau vivant.*" The distinctive mark of a person of the *type d'érudit* is the tendency not to describe the object at all, but to state his *knowledge about* it. The object of his attention is not the impression, but the associated or remembered facts called up by it. The last type, the *type émotionel* or *imaginatif*, attempts to describe—and therefore presumably also to apperceive—not so much the visible details of the object as its emotional meaning or its mood. A person of this class inclines, therefore, first, to grasp this aspect, and then to select only such details as help to reinforce the mood or its emotional appeal. Descriptions by this type are consequently equally centralised as those of the *type observateur*, but usually less rich in accurate detail and much more emotionally coloured. On the other hand, they are fuller of ideas and show more literary finish[1]. It must, of course, be remembered that these tests were carried out with children which detracts from the aesthetic value of the results, and that they were not undertaken from an aesthetic point of view at all. But, if we can trust our knowledge derived from other sources and supported by general considerations, it would seem that of these four types two at least, the *type observateur* and the *type émotionel*, have an aesthetic significance. I am confirmed in this belief by a set of experiments by F. Müller[2]. They were admirably arranged and were preceded by a trial experiment with adults, which enabled him to exercise a criticism upon the different types of his subjects and supplied a kind of aesthetic scale according to which he assessed the apperceptive types. He used Binet's type-distinctions which he slightly elaborated, and places, as it seems to me justly, both the *type descripteur* and the *type d'érudit* low in the aesthetic valuation.

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[1] Except the above-mentioned experiments of Külpe and Dessoir and those of v. Ritook: "Zur Analyse der aesthetischen Wirkung auf Grund der Methode der Zeitvariation," *Ztsch. f. Aesth.* v. 356, 512, and some experiments undertaken from the general psychological point of view which will be laid under contribution in the sequel.

[2] M. Dessoir: "Das Beschreiben von Bildern," *Ztsch. f. Aesth.* VIII. 460.

[3] This is a special case of the non-equivalence of the method of impression and the method of production which I have pointed out elsewhere (this *Journal*, III. 425–7).

[4] A. Binet: *Étude expérimentale de l'Intelligence*, Paris, 1903.

Another classification of direct bearing upon Aesthetics is the distinction between 'analytic' and 'synthetic' apprehension. The difference, rather vague, has long enjoyed general vogue, though it may be questioned whether either analysis or synthesis is ever completely absent from any act of apprehension. The interest lies rather in the different degree of emphasis given to the one or the other, and in the possibility that such differences are perhaps peculiar to the apprehension of particular objects or to the apperceptive habits of certain types of persons. "The process of perception...takes place in two stages. There is first (*a*) an immediate interpretation of the object as a whole and next (*b*) an analysis of this vaguely apprehended whole into its component parts.

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[1] An interesting confirmation of the *type observateur* and the *type émotionel* and of their aesthetic possibilities is furnished by Tolstoy (Сочиненія, IV. 176–8, 1903). A translation of part of this passage is to be found in P. Birukoff, *Tolstoy, his Life and Work*, I. 337, Heinemann, London. The occasion for Tolstoy's observations was a lesson in composition given to the peasant children of Yassnaya Poliana.

[2] F. Muller: *Aesthetisches und ausser-aesthetisches Urteilen des Kindes bei der Betrachtung von Bildwerken*, 1912. Müller expanded Binet's four types into six: (1) descriptive, (2) observing, (3) emotionally-descriptive, (4) emotionally-observing, (5) eruditely-descriptive, and (6) eruditely-observing (pp. 52–3).

Subjects of all ages show a striking uniformity in the first process, whereas in the second there was no less striking variability." These words are taken from a set of experiments[1] on perception undertaken from a general point of view and carried out by means of short exposures of lantern-slide pictures of an average of 1/13″ in the hope of thereby delaying the process of perception and recognition. These exposures were repeated as often as the subject wished, until he felt that he was unable 'to get any more out of it.' The failure to delay recognition or interpretation concords entirely with the earlier experiments of Külpe. The second step again suggests differences between individuals in their mode of apprehension which, unfortunately, do not appear with sufficient clearness in the records, but resemble the distinctions between Binet's *type descripteur* on the one side and his *type observateur* and *émotionel* on the other. The subjects appear to have followed two distinct procedures: "some continued (after the general recognition of the object) to see each picture as a whole except when some outstanding feature happened to fall in the focus of their vision....Other subjects made a more or less systematic search for details[2]." According to the experimenter it was especially the scientists who were distinguished by their systematic and patient analysis of every detail, quite irrespective of its importance for the whole or its intrinsic value, prompted by an impersonal curiosity to solve every obscurity and by an equally impersonal passion for completeness. Some asked for fifty or sixty exposures before they were satisfied, while subjects of the other type contented themselves with a tenth part of this number. Unfortunately the special object of these tests renders them hardly directly applicable to aesthetic purposes. Especially the lack of critical distinctions between the subjects, divided simply into scientists and non-scientists without reference even to their psychological differences in imagery, etc., and the hardly disguised bias of the experimenter in favour of the scientific mind, detract from their usefulness.

A peculiar defect from the aesthetic point of view was the choice of the pictures themselves. It would be of great value to ascertain whether objects of different character would be able to force upon the subjects an analytic or non-analytic apprehension, irrespectively of their usual apprehension and even against their individual apperceptive habits. In particular it would be interesting to discover whether the one or the other mode corresponds to aesthetic and non-aesthetic apprehension, assuming this difference to exist. But none of the slides used lent themselves easily to aesthetic appreciation, which was anyhow rendered almost impossible by the exclusively perceptive adaptation required by the tests. Most of the pictures showed more or less simple objects like animals or complex things without aesthetic interest. The only slides arousing more personal interest were two 'incident pictures' representing 'humorous incidents' of a very mild description and very inferior execution. It is interesting to note that several subjects called attention to the fact that their apprehension of these slides was of a type different from that of the other pictures. "I feel here," says one of them, "a marked difference of perception. In the engine picture it was a question of details. Here it is a question of the sense of a story, for which details are unnecessary"; and another stated that for him "the parts hardly exist in the story picture. Details would be a nuisance, so I keep to the story interest[1]." These differences make it probable that similar tests with different slide material and different adaptations might produce observations of interest for Aesthetics.

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[1] F. Smith: "An Experimental Study of Perception," this *Journal*, VI. 325 ff.

[2] *Ibid.* 329.

This is rendered all the more likely by some tests undertaken by M. Dessoir on the *Description of Pictures*[2]. All his subjects (10) were students of the history of Art. Three exposures of 1″ each were given of slides made from paintings of standing, though unknown to the subjects. After each exposure they were asked to write down a description of what they had seen. The result showed that all started with a general apprehension of the contents and especially with the disposition of the main elements of the pictures. "Even at the first immediate impression the pictures are determined with more or less certainty as belonging to a particular period of Art history (Dutch, Renaissance, 18th century, Modern), and as displaying a particular technique (etching, oil-painting, etc.), and are apprehended in their general appearance[3]." "The picture is apperceived in certain general outlines, so that those elements without which it would be artistically unintelligible, offer themselves for description[4]," and "it is apprehended from the start as a whole, within which some details emerge at once with perfect clearness[5]." "The subjects had the feeling at the first, or at the latest with the second, exposure that they had apprehended all that could be perceived in a momentary exposure[6]." Even the second and third exposures do not seem to have led to so purely analytic an apprehension as occurred among the scientists in the previously mentioned tests. No doubt details were gradually grasped with greater clearness, but—remarks Dessoir—"in spite of all the detailed apprehension as the description proceeds, the whole remains consciously present. Even the smallest detail is apprehended not for its own sake, so to speak, but in connexion with the whole[1]."

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[1] F. Smith: *loc. cit.* 335.

[2] M. Dessoir: "Das Beschreiben von Bildern," *Ztsch. f. Aesth.* VIII. 440.

[3] *Ibid.* 446. This is undoubtedly due to the particular bias of his subjects.

[4] *Ibid.* 447. [5] *Ibid.* 449. [6] *Ibid.* 450.

The tendency to apprehend parts or single details of a picture and to base the appreciation upon these, appeared in some experiments of Miss Calkins[2] as a peculiarity of children under nine years of age, and she summarises in the result the principal difference between young children and adults as the apperception of parts and of the whole respectively[3].

One point emerged accidentally from Dessoir's experiments which should certainly be examined more closely: some of his subjects seem to have apprehended first the *formal features* of the picture, the composition, general shapes and outlines, the grouping and the relation of main parts to each other, while others were conscious of the *content represented*, rather than of the formal aspect[4]. This apperception of formal qualities is corroborated by the tests of Külpe[5] and v. Ritook[6]. The last named even inclines to regard the difference as one of apperceptive type[7]. It seems desirable to obtain fuller information on this point by experiments specially designed to test this question, as the short exposures with which all these researchers worked may of themselves have forced the attention to formal characteristics. The difference between subjects may, however, be at the bottom of differences of apperception of simple spatial forms to which Katz[8] has drawn attention. He discovered at first accidentally and then proved by experiments, that persons differ in the manner of apperceiving such simple figures as triangles or two-dimensional representations of cubes, pyramids, cylinders, etc. Some interpret and apprehend suitable line-complexes as faces, or plain figures as plastic bodies, while others take them merely as unmeaning collocations of lines or as two-dimensional figures. Similar differences were found by Karpinska in the apperception of three-dimensional objects[1]. Katz suggests a distinction between 'peripheral' and 'central' types of apprehension, according as the visual impression is apprehended as such, or is modified by subjective additions leading to interpretation.

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[1] M Dessoir: *loc. cit.* 453.

[2] M. W. Calkins: "An attempted Experiment in psychological Aesthetics," *Psychol. Rev.* VII. 586–7.

[3] *Ibid.* 590–1. The same observation was made by Müller (above).

[4] M. Dessoir: *l.c.* 447.

[5] O. Külpe: *l.c.*

[6] v. Ritook: *l.c.*

[7] *Ibid.* 371–2. v. Ritook observed that one of her subjects habitually based his appreciation on the formal side of pictures. Müller observed that an awareness of formal features and of the artist's intentions and treatment occurred only with subjects having an interest in, and some experience of, Art, but was lacking in aesthetically uneducated subjects.

[8] D. Katz: "Über individuelle Verschiedenheiten bei der Auffassung von Figuren," *Ztsch. f. Psychol.* LXV. 161 ff.

As the result of an extensive series of individual tests of over 100 persons on their appreciation of single colours and simple colour-combinations[2], I found the existence of four clearly distinguishable types of apperception which I called respectively the 'objective,' 'physiological[3],' 'associative' and 'character' types. The peculiarity of the objective type is an impersonal attitude to the colour and a tendency to base appreciation upon its purity, saturation, luminosity, and in general upon its degree of conformity to a preconceived standard. The second type apprehends colour as an influence upon the personal mood and, presumably, the organism of the individual: colours are cheering or depressing, warm or cold, stimulating or soothing. For the third or associative type it is the power of the colour to call up associations or memory images of past experiences which determines the appreciation of the colour. In the eyes of the character-type, finally, colour possesses a mood, emotional character or temperament, even a personality. Colours are cheerful (as distinct from cheering), sad (as distinct from depressing), active, serious, reserved, reflective, brutal, jovial, funny, treacherous, stealthy, etc. Allegiance to one of these types—and everyone belongs to one or other—determines both the range and nature of the colour-preferences and the basis for the use of special criteria in dealing with colour-combinations. The types appear to be not merely momentary attitudes of the subject, but fundamental and permanent modes of apprehending and appreciating colour.

These same types were found to exist in the appreciation of musical tones, as resulted from a set of experiments on "Individual Differences in Attitude towards Tones[1]." It would appear from these tests that the term originally proposed for the second of the above types (the 'physiological') is too narrow and that 'intra-subjective' is a better description of that attitude. In addition, the differences of the material added to the results of the experiments with colours a number of most suggestive observations concerning the range and elaborations of the typical attitudes.

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[1] L. v. Karpinska: "Experimentelle Beiträge zur Analyse der Tiefenwahrnehmung," *Ztsch. f. Psychol.* LVII. 1.

[2] Edward Bullough: "The 'perceptive Problem' in the aesthetic Appreciation of single Colours," this *Journal*, II. 406 ff.; "The 'perceptive Problem' in the aesthetic Appreciation of simple Colour-combinations," this *Journal*, III. 406 ff.

[3] After the experiments of C. S. Myers (see p. 87, note 1), I would adopt the term 'intra-subjective' proposed by him, in place of 'physiological,' which was at best a makeshift and was found by him to be too narrow, when applied to musical experiences.

A last distinction of forms of apprehension has been pointed out by Aveling[2] between what he calls 'symbolic' and 'a-symbolic' apperception. While the 'a-symbolic' type apprehends objects and their meanings as individual, concrete things, the 'symbolic' subjects incline to see in them types or representatives of the species to which they belong. The tendency to see in the picture of a horse, not this individual particular horse, but 'Horse' in the abstract, depends naturally in part upon the kind of picture[3], but appears also and perhaps in the first place to be the outcome of a fundamental symbolising disposition of the subject.

It is clearly a problem of aesthetic research how and to what extent these various apperceptive classifications are to be reconciled. Binet's four types, the apperceptive habits described as analytic and synthetic, active and passive, objective and subjective, peripheral and central, symbolic and a-symbolic, and my four types show sufficiently certain resemblances to each other. In attempting to reduce them to a simple grouping, it has to be remembered that several of these types emerged not in reference to specifically aesthetic tests, but as the results of general psychological experiments. It is very probable, therefore, that several such distinctions may not belong to the aesthetic sphere at all.—Secondly, it is essential that proper criticism should be exercised towards the subjects themselves. While Dessoir's tests, for instance, dealt with subjects to whom the aesthetic attitude was familiar and in face of pictures habitual, Smith's experiments give no indication how far, if at all, his subjects reached an aesthetic appreciation of the slides. In aesthetic experiments it is desirable that not all the subjects should have the same habitual outlook, aesthetic or otherwise, so that the distinction between aesthetic and non-aesthetic behaviour should appear with sufficient clearness. At the same time, like must be compared with like and not with unlike. It is therefore necessary for the experimenter to be in a position to exercise a critical discrimination, based on general psychological, aesthetic and personal knowledge, between his various subjects. This discrimination will enable him to give due weight and value to the evidence supplied by subjects of a known aesthetic receptivity, compared with others to whom the aesthetic attitude is admittedly difficult or non-habitual[1].

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[1] C. S. Myers and C. W. Valentine: "A Study of the Individual Differences in Attitude towards Tones," this *Journal*, VII. 68 ff.

[2] F. Aveling: "The Relation of Thought-process and Percept in Perception," this *Journal*, IV. 227.

[3] *Ibid.* 227.

With such an evaluation of the testimony of subjects it is not improbable that the apparent diversity of types will prove much smaller than seems at first sight. At the same time the complexity of even the mere apprehension of an aesthetic object is evidently much greater than was previously suspected. The differences in aesthetic judgments were well known; varieties of associations were hardly a matter of surprise; the fluctuations in Empathy and the divergencies of its psychological working became evident as soon as analysis concerned itself with these processes. But the existence of apperceptive types has now introduced similar complications into the pre-empathic stages of appreciation. The one guiding principle, as a criterion of the aesthetic quality and value of the different forms of apperception, appears to be the conception of the *unification of the percept and the perceptual processes*. It is true that this is something of an *a priori* principle. But its importance appears repeatedly: in Dessoir's tests[2], in Müller's observations on children[3], on several occasions in v. Ritook's experiments[4], it came out in a peculiarly striking, because quite unexpected, manner in the experiments of v. Karpinska[5] on the perception of (three-dimensional) depth, where the plastic apperception of a really plane figure was said by some of the subjects to be far more unified and therefore aesthetically more effective than the view of the same figure as a plane design, which appeared scattered and disordered. It seemed to Miss Calkins[6] the main difference between an immature and a developed appreciation and it has been used for the evaluation of perceptive types in my colour experiments[7], supported by similar observations of Valentine[1]. The value of unification of the aesthetic experience, subjectively as a certain homogeneity or unity of the processes of appreciation, and objectively as the unification of the object by composition, content, or emotional and associative import, is so extensively attested by experience that it seems difficult to over-rate its importance. The various means by which it is attained, and the range of its application, especially to temporal forms, are urgent problems for further experimental research[2].

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[1] This is a general criticism of employing subjects whose knowledge or interest leads to a uniform mental attitude. It affects to some extent even v. Ritook's experiments. It does not affect Dessoir's tests, since his object presupposed a certain uniformity of attitude on the part of his subjects. On the other hand, it renders experiments such as those of Utitz (*Grundzüge der aesthetischen Farbenlehre*, 1908) practically valueless, as I have pointed out elsewhere (this *Journal*, III. 444, footnote 1). Admirable in the matter of discrimination are Müller's experiments, as already stated.

[2] Dessoir: *l.c.*

[3] F. Müller: *l.c.*

[4] v. Ritook: *l.c.*

[5] v. Karpinska: *l.c.* Cf. also Bühler, *Die Gestaltwahrnehmung*, 1913, 176. Here Prof. Kulpe, acting as one of his subjects, contrasted what he called the 'aesthetic' apperception of the figures as synthetic, with the 'geometrical' or analytic apperception.

[6] Calkins: *l.c.*

[7] Bullough: *l.c.*

At this point I may perhaps be permitted a short digression, as its substance is so closely connected with the question of apperception. Though the apperceptive differences are of relatively recent discovery, the general importance of the process of apprehending an aesthetic object was realised much earlier and has occasionally been made the basis for general aesthetic theories. Two groups of such theories may conveniently be mentioned here. We may call those of the one 'intellectualistic,' of the other 'functional' theories.

The intellectualistic theories need not detain us. Their essence is that an object is aesthetically effective in proportion to the ease with

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[1] C. W. Valentine: *The Psychology of Beauty* (The People's Books), rev. ed., 1919, 101.

[2] By way of illustration, the following correspondences might be suggested:

(*a*) My four types in reference to Binet's:

objective	... *descripteur; érudit.*
intra-subjective	*observateur* plus *émotionel.*
associative	... *observateur* plus *émotionel* plus *érudit.*
character	... *émotionel* plus *observateur.*

(*b*) Binet's types in reference to mine:

descripteur	objective (1).	
observateur	character	associative.
émotionel		
érudit	objective (2)	

A considerably better correspondence is obtainable between my four types and Muller's six, expanded from Binet's four.

Another grouping is the classification under Unification and Dissociation as follows:

Unification		*Dissociation*
synthetic		analytic
central		peripheral
subjective		objective
observateur		*descripteur*
	émotionel	
	érudit	
character		
associative		associative
	intra-subjective	
objective		objective.

which it is grasped. This is at bottom the standpoint of all theories resting upon formal features, ratios, proportions, the golden section, etc. The recent experiments of Bühler[1] on the perception of spatial forms have indeed shown the importance of proportion for the apprehension of linear complexes. But what is open to question is whether such a facility of apprehension and the consequent satisfaction is necessarily aesthetic, as it clearly occurs not merely in the aesthetic sphere, but just as constantly outside it. That ease of apperception—even the purely intellectual facility in apprehending meanings and historical and technical significance—may be a source of satisfaction and of pleasure, can hardly be contested, but the pleasure is by no means generally aesthetic. When it is, the fact can presumably be accounted for by the existence of an aesthetic adaptation which colours all the subsequent processes[2].—The once prevalent idea that familiarity is a source of aesthetic satisfaction, is of the same category, but manifestly untrue. For familiarity, as we know, more frequently breeds contempt.

The functional theories assert that certain objects are aesthetically pleasing, because they offer the best conditions for the easy and natural functioning of the physiological processes required for their apperception. The most prominent theory of this kind is the so-called 'eye-movement' theory which goes back to the eighteenth century and secured a fresh lease of life in the second half of the nineteenth through the 'Wundt-Lamansky law' of eye-movements. The eye-movement theory maintains that owing to the disposition of the muscles of the eye-ball certain movements are more easily carried out than others; and that therefore lines which 'guide' the eye in the direction of these naturally easy movements are more pleasing than lines which cause the eye to execute more unnatural and difficult movements. The directions of easy movements are, according to the Wundt-Lamansky law, straight vertical and horizontal, and curved oblique lines. Sudden changes of direction, or straight oblique lines conflict with the muscular apparatus of the eye-ball and tend to produce torsions and uncertainties in the eye-movements. The consequence for Aesthetics would appear to be that linear forms should employ *straight* verticals and horizontals, but *curved* obliques; undulating lines, but not patterns involving constant changes of direction, like zig-zags; and that such figures would be pleasing as a result of the facility with which 'the eye follows' their lines. For the same reason symmetrical forms are said to have the advantage of demanding a balance or correspondence of eye-movements for the apprehension of the right and left halves[1].

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[1] Bühler: *l.c.* (see p. 88, n. 5).

[2] Cf. v. Ritook: *l.c.* 365.

The theory rests, of course, on the assumption that the eye follows the lines and contours, as do all those unfortunate metaphorical expressions speaking of 'the eye being guided or running along certain lines.' The eye does nothing of the kind. The whole assumption is false. On the contrary; the extremely painstaking and elaborate experiments carried out by Stratton[2] and Judd[3] have shown that the eye does not follow the lines, not even when the subject is told to follow them with his glance and is quite convinced that he is doing so. "In examining the negatives (of photographs taken of the positions of the eye)—says Stratton—one is struck by the almost grotesque unlikeness between the outline observed and the action of the eye in observing it. For the most part the eye moves irregularly over the figure, seeking certain points of vantage from which the best view of the important features may be obtained. And these positions are marked by the eye momentarily resting there...But even these points of rest are not so arranged as to supply of themselves a rough sense of the form perceived[4]...." The same results were obtained from experiments with symmetrical forms: "As regards the question of symmetry of the movements of the eye, in some instances the negative (photographic record) displays a rude balance...But it must be said that even the most symmetrical of these records can be called so only by courtesy, for they signally lack that exactness of correspondence of part to part which we demand in the objective form[5]."

With these observations the eye-movement theory may be regarded as definitely disposed of. The same fate may be anticipated for various other theories which make the easy functioning of muscular or organic action in the apprehension of beautiful objects the basis of aesthetic values, though, as Stratton remarks[6], some of these may be more difficult to disprove by actual observations.

A much more elaborate theory, combining in a sense the functional and intellectualistic conceptions, is the 'apperceptive principle' of Lipps[7].

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[1] See, for instance, Baldwin Brown: *The Fine Arts*, 212–4, London, Murray, 1902.

[2] G. M. Stratton. "Eye-Movements and the Aesthetics of visual Form," *Philos. Stud.* XX. 336 ff.; "Symmetry, linear illusions and the Movements of the Eye," *Psychol. Rev.* XIII. 82.

[3] C. H. Judd: "Studies in Eye-Movements," *Psychol. Rev. Monograph Supplement*, VII. 1; *Yale Psychol. Stud.* I. 1.

[4] Stratton: *Psychol. Rev.* XIII. 94.

[5] *Ibid.* 94.

[6] *Ibid.* 95.

[7] Th. Lipps: *Aesthetik*, I. 1903, 10 ff.

He tries to establish a connexion between pleasure, value and apperception by treating the last named as the principle of pleasure (in the widest sense). "Pleasure arises to the extent to which a psychical process (or its object) finds favourable conditions for its apperception (using 'apperception' in the somewhat specialised sense of 'appropriation' or incorporation into the general psychical life of a person)[1]." Apperception is, so to speak, the mutual fitting into each other of the object and the conditions of its apprehension, and is in so far a source of pleasure. Since "there is the tendency of the soul to comprise all data, all multiplicity into a unity or uniform whole in the act of apprehending or attending or apperceiving[2]"—a law which he calls the 'principle of unity[3]'—objects which meet this tendency halfway; because they offer objectively a unity, are apperceived with greater facility and possess therefore a higher aesthetic value than others which do not offer the same favourable conditions. Hence, Lipps contends, the aesthetic superiority of formal features, rhythms, symmetry, balance, contrast, or proportion, which he with great ingenuity and wealth of illustration brings under the common heading of 'unity in variety'[4].

In spite of the emphasis laid upon the pleasure-value of the aesthetic experience, Lipps' account, apart from the peculiarities of his terminology at times, seems not unattractive. Unfortunately he presses the apperceptive foundations of his system into the exclusive service of his empathy doctrine which of all the empathy theories is perhaps the least happy in formulation.

ADAPTATION.

If relatively so little is known of apperception, we are almost entirely in the region of hypothesis concerning adaptation. There is no doubt that adaptation actually occurs, of course. Our ignorance refers mainly to the special conditions under which it takes place, its character and composition and the question to what extent different types of it can be distinguished. This lack of information is largely due to the fact that adaptation is by its very nature difficult to deal with experimentally and that it seems to involve some of the obscurest factors of 'temperament' and 'personality'; chiefly, however, to the accident that it has never yet been studied as closely as the other stages and factors of aesthetic appreciation. Yet there is a growing sense that here, if anywhere, we may find that bedrock upon which the whole superstructure of aesthetic experience—of appreciation as well as artistic production—ultimately rests.

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[1] Th. Lipps: *l.c.* 11. [2] *Ibid.* 12. [3] *Ibid.* 19.

[4] In accordance with his bias against all 'empirical' factors, Lipps excludes expressly the *intellectuale Lust*, arising from an intellectual knowledge of unity (*Aesthetik*, I. 22 ff.). The obvious difficulty presented by simple objects (colours, tones, etc.), he tries to meet by the desperate expedient of grouping even these under the principle of 'unity in variety' by reference to the vibrations constituting colours and tones (*Aesthetik*, I. 27 ff.).

The interest in adaptation is of relatively recent date. In 1905 Segal[1] observed in carrying out a set of experiments with simple linear forms that the same subject found oblique straight lines sometimes pleasant and sometimes unpleasant, occasionally on one and the same day. The explanation of this response was found to be that the subject regarded these oblique lines, sometimes as imperfect horizontals or verticals (and found them unpleasant as failures to be either one or the other), sometimes as a flying arrow. This latter apperception always produced a pleasant effect. The question arose, what prompted the subject to adopt now this, now the other interpretation? Apart from the natural ambiguity of simple forms, Segal saw the reason, first "in the general, hardly definable, fundamental character of the individual, that which we call his individuality or personality," and secondly in "the momentary mood and adaptation (*Bewusstseinslage*), immediately preceding the apperception of the object[2]."

This is to my knowledge the first explicit reference in aesthetic literature to any such fact as adaptation. However imperfect and contestable in details, Segal's observation was a discovery of the greatest value. It is true that he believed this adaptation as well as the interpretation based upon it to lie outside the sphere of the aesthetic experience, to be a 'pre-aesthetic' process common to all apprehension. Segal's view would, I think, hardly be shared at the present time, since Empathy, the real object of his experiments, has lost considerably in vogue, and the pre-empathic stages of appreciation have attracted more attention. Adaptation, in the sense I venture to give to it, is undoubtedly connected with the general temperament and personality of an individual. The personality as such is, of course, as Segal said, 'pre-aesthetic.' But the adaptation, though a reflexion or manifestation of the personality, belongs unquestionably to the aesthetic sphere. As the result of my colour-experiments and the discovery of the perceptive types, I believe that adaptation as well as apperception are not merely a colourless basis for the later stages of appreciation; adaptation is not simply a gateway through which one walks into the garden of aesthetic delight. It is rather a coloured glass through which one sees all objects steeped in a special colour, and this colour is precisely the aesthetic quality which the objects seem to display. It is, irrespectively of the objects themselves, an *aesthetic* adaptation and represents the whole groundwork on which the aesthetic experience rests. It is needless to say that this is a view so different from that of Segal that it would be unfair to use it as a criticism of his original conception.

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[1] J. Segal: "Über die Wohlgefälligkeit einfacher räumlicher Formen," *Arch. f. d. ges. Psychol.* VII. 53 ff.

[2] *Ibid.* 108.

Before explaining more in detail the significance of adaptation for aesthetic theory thus hinted at, a few words are needed to indicate its nature. In a general way, adaptation implies, positively, the attending to the various aspects presented by the object (either spontaneously or by an act of voluntary attention) and the opening of all the channels of reminiscence, associations, historical and technical knowledge, inferences, emotional resonances, organic sensations, etc., in so far as they may serve to impart an adequate meaning to the object and to lead to an adequate interpretation of it on the lines predetermined by the artist. Negatively, it involves the cutting out of all irrelevant trends of thought, extra-aesthetic points of view, the selection of valuable associations and the suppression of useless or disturbing dispositions, especially of such as by their intensity or nature would lead to deviating associations, purely personal interests, or reminiscences splitting the interest in the object or destroying the unity of attention and appreciation.

It is one of the tasks of experimental research to investigate the chief characteristics of aesthetic adaptation, and by way of suggestion, I venture to put forward four features which I believe to be peculiar to it. In so doing I hope to make clearer the nature of aesthetic adaptation as it presents itself to me, and the function which I would attribute to it in aesthetic theory.

1. Like all adaptation, it involves a large element of expectancy. But this expectancy is curiously vague and undefined. It is true that an adaptation to a special art or even type of work to which the prospective aesthetic experience belongs sets certain limits to this vagueness. We know whether to expect a comedy or a tragedy when we go to the theatre, a sonata or an oratorio when we attend a concert. But it is not —and this seems to me an important characteristic, complicating this indefiniteness—the *typical* experience, but the *individual*, concrete experience which is the object of expectancy[1]. It is not Tragedy that

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[1] This would run counter, as stated immediately below, to *normal* adaptation which is for typical, generic experience rather than for individual. Cf. the general rule (stated by Külpe, *Grundriss der Psychologie* (1893)), that generic experiences are better remembered than individual. This also affects reproduction.

we go to see, nor even 'Shakespearean Tragedy,' but this or that particular tragedy, on this particular occasion—whether we have ever seen it before or not.

2. In fact, our adaptation to the individual and concrete experience is the more effective and adequate, the less it operates with preconceptions and typical standards, with the possible affinities or resemblances of the work, or with its concordance with habit or tradition. While the value of ordinary experience lies largely in its confirmation of our expectations, it is characteristic of aesthetic experiences to surprise us by their originality. This seems to be the result of the combination of the marked individualistic expectancy of our prospective experience with —however paradoxical it sounds—the indefiniteness of our adaptation. This vagueness, the very reverse of dogmatism, but an open-mindedness, even a kind of trustful humility is described somewhere by Schopenhauer, by likening our attitude to that of a humble subject approaching his king and waiting in silence until he should deign to speak to him.

A failure in these two respects is well known as the cause of many a miscarried effect. A pronounced typical expectancy either fails to see the concrete novelty or misses the—wrongly expected—correspondence with traditional or habitual forms. Dogmatic standards and demands are only another form of the same fault of adaptation, the result of prejudice, narrow-mindedness or merely of set habits and immobility of mind.

3. In the third place, the general feeling-tone of our adaptation is markedly pleasant. But it is important to note that this pleasantness is centred not upon the expectation of a *personally pleasant* experience, but upon the *value* of the prospective experience.

4. Closely connected with this last point, I must mention the perhaps most important feature of the aesthetic adaptation, namely its impersonal or rather non-personal character.

There is a fairly general agreement about this characteristic, although there is as yet no concensus on its accurate description or its full significance. Personally I incline to regard this as the most important and fundamental aspect of the aesthetic, as distinct from other, adaptation. Its main feature is a divorce from all directly practical needs and functions and from the personal relevance of the object: it is neither agreeable (as a personal affection), nor useful (as fulfilling a practical function), nor directly ethically valuable (as serving a socially sanctioned, remotely personal purpose). At the same time, our adaptation is not *im*personal, in the sense of scientific impartiality or mere intellectual curiosity. It is rather a non-personal relation to the object; its significance does not affect me in my everyday experience, yet it does not lose touch with either the emotional sphere or a generally human interest; and though the experience has a certain unreality or, if you like, fictitiousness about it, it yet does not fail to obtain a full response of the personality to which it appeals. This curious dualism of attitude, a paradoxical combination of impersonal and intensely personal reaction, I have tried elsewhere to describe as 'Distance[1].'' Others have tried to formulate these puzzling facts by reference to 'assumptions[2]' in place of existential judgments, or an attitude of 'supposal' or make-believe, as the basis of aesthetic response. This view lays successful stress upon the imaginative element involved in the aesthetic attitude, but is held to find difficulties in the occurrence of an aesthetic appreciation of Nature and of actual events in real life. The theory of 'imaginative emotions' (*Phantasiegefühle*)[3] attempts to meet this objection and to obviate some obscurities in the 'assumption theory.' Another view treats the aesthetic attitude as the result of a 'play-adaptation.' This attempt has certain drawbacks, inherent in the correlation of Art and Play, a theory of Art which goes back to the eighteenth century but was revived in the last decade of the nineteenth century. The 'play-adaptation' has possibly, as the opposite to a 'reality-adaptation' (*Wirklichkeits-Einstellung*), to be distinguished from a 'non-serious-adaptation' (*Nicht-ernst-Einstellung*) as has recently been pointed out[4].

All these attempts to define this characteristic feature of aesthetic adaptation is a great step in advance over the old 'illusion' theories based on deception or probability or on the opposition between the work and some 'copied' reality. Whatever the ultimate formulation will be, I feel sure that we shall find here one of the marks, perhaps *the* mark, distinguishing aesthetic adaptation. It may be this feature which will prove to be the condition of all the subsequent elaborations and colourings of aesthetic, as distinguished from other types of, experience. It is certainly this non-practical, non-personal character of adaptation which is responsible for the seemingly unreal quality of the emotions of Empathy and for the 'semblance' of the whole experience. It is the 'filtering' process exercised by it which permits the associations, freed from their purely personal significance, to enter as ingredients into aesthetic appreciation and to enrich meaning with a quasi-personal interest. It establishes the distinction between the beautiful and the agreeable, marks off the tragic from the sad and the humorous from the merely ludicrous. It is, in short, that unifying element which renders aesthetic appreciation homogeneous and imparts to it its singleness of point of view, quality and appeal.

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[1] Edward Bullough: "Psychical Distance as a Factor in Art and as an aesthetic Principle," this *Journal*, v. 87–118.

[2] A. Meinong: "Über Annahmen," *Ztsch. f. Psychol. u. Physiol. d. Sinnesorg., Ergänzungsband*, 2, 1902; S. Witasek: "Zur psychologischen Analyse der Einfühlung," *Ztsch. f. Psychol. u. Physiol. d. Sinnesorg.* xxv. 4 ff.; *Grundzüge der Aesthetik*, 1904.

[3] R. Saxinger: "Über die Natur der Phantasiegefühle u. Phantasiebegehrungen" (in Meinong's *Gegenstandstheorie*, 1904). Saxinger tries to show that *Phantasiegefühle* are emotional states *sui generis*, not as Witasek maintained and Meinong implied—wrongly and unintentionally, according to Saxinger—always dependent on or equivalent to *Annahmegefühle*.

[4] M. Geiger: "Das Problem der aesthetischen Scheingefühle," *Ber. d. ersten Kongresses für Aesthetik*, Berlin, 1913, 191.

It is important to distinguish from the question of the nature and constitution of aesthetic adaptation, the problem of its typical behaviour and distribution.

As research found the different types of apperception to occur with varying frequency among different types of persons, so it is clear—even from our superficial every-day experience—that aesthetic adaptation occurs in various degrees of frequency, stability and permanence in different individuals. And as different types of persons have come to be distinguished by their habitual mode of apperception, nothing would seem more probable than that persons would be found to fall into different classes according to the type of adaptation habitual to them. The perceptive types seemed to all appearance to be permanent. The same persons showed after a lapse of years the same characteristics of apperception and, allowing for the difference of another medium, the peculiar features of their type also in musical experiences. Even the possibility that these types are inherited is not to be altogether dismissed. All this goes to show that a special apperceptive habit is a definite and possibly fundamental asset of its owner. Without suggesting a necessary correlation between perceptive types and adaptation types, I cannot but think that similar divisions into types of adaptation would be possible, once sufficient experimental data were available.

At the same time the questions of spontaneity and stability of adaptation, both in concrete instances and as typical characters of different groups of persons, might find their answer. That adaptation can be attained voluntarily is well known; but in many people it is presumably a spontaneous process, in proportion as the aesthetic adaptation is natural or habitual with them. It is a special object of research, how far disturbing factors can be either voluntarily overcome or are spontaneously set aside. This leads to the question of stability of adaptation, *i.e.* of the greater or lesser facility of different persons to maintain an aesthetic adaptation for any length of time. In reference to both these questions it must be borne in mind that adaptation may be secured or assisted by the object, by its power to *compel* aesthetic adaptation. This is, of course, what makes an object into a work of Art or a source of aesthetic appreciation. The temporal arts in particular dispose of special means to initiate an adaptation, by 'introductions,' opening chapters, the 'exposition' of a play, an overture, etc. This advantage over the spatial arts which cannot prepare the spectator is, however, balanced by the disadvantage of requiring the maintenance of an adequate adaptation for a considerable length of time. It is here that the adaptation is exposed to fluctuations, deviations and apparently especially to oscillations between the object and the subject's self and his reactive feelings.

If aesthetic adaptation proves to be as permanent and as fundamental a form of behaviour as I believe it to be, a new prospect opens out for aesthetic theory. In that case aesthetic adaptation, with all its profound importance for the subsequent stages of the aesthetic state, comes to represent a whole system of dispositions and tendencies orientated towards aesthetic experiences, which is more or less habitual to certain persons, and less frequently realised by others for whom it is attainable only in response to stronger, more explicit stimulation. This disposition or 'polarisation' towards aesthetic experiences constitutes the 'aesthetic consciousness'—in the same sense as we speak of a religious 'consciousness'—within which and through which objects and experiences are aesthetically appreciated. That this kind of special outlook exists, admits of no doubt as a matter of broad, every-day experience. Older Aesthetics used a stock-illustration to show the 'subjective' nature of beauty, by describing the view taken of a landscape by various hypothetical visitors, a shop-keeper, an engineer, an estate-agent, a medical man, a prospector, a farmer, and contrasting their attitude with that of the painter. The difference of outlook between the painter and all the rest is the difference between the 'aesthetic' and the 'practical consciousness.' It is a difference, if I may use my own terminology, guaranteed by the 'distance' of the aesthetic adaptation and attitude[1].

The same guarantee maintains the distinction between the ethical and scientific attitudes on the one side, and the aesthetic attitude on the other. Both the practical value of moral conduct and the impersonal curiosity of science stand opposed to the balance of distance of the aesthetic consciousness. It is time at last to discard the time-honoured confusion of goodness, truth and beauty.

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[1] Cf. for an indication of the differences of attitude, the opening chapter of *L'Intelligence sympathique* of G. Finnbogason, Paris, 1913.

These and similar distinctions belong really to the domain of a Philosophy of Art. The important point to seize at the moment is that until the conceptions with which Philosophies of Art are wont to operate are illuminated by actually and accurately observed experiences of many persons, instead of being vaguely apprehended and rashly generalised personal introspections of their authors, little good will be done by interminable discussions of such topics. All I desire to point out here is that we have reason to believe that there *is* a distinction which imparts to the aesthetic consciousness a character and significance of its own, irreducible to the value of the other attitudes. It is the concern of aesthetic theory and experiment to define and analyse this consciousness, to ascertain the conditions of its realisation and the range and typical features of its occurrence. If this attempt proves successful, theory will have attained to a fundamental generalisation in Aesthetics. For this consciousness, as a mental state *sui generis*, is the level of psychic life which must be reached and maintained, if the aesthetic state is to be realised. Appreciation will then not be dependent upon the presence of this or that mental process, upon pleasure or emotion, upon empathy or association or imagery or organic sensations. No; any mental state, from the obscurest organic feeling to the most abstract intellectual operation, may form the object of aesthetic appreciation—*provided it occurs within and on the level of the sphere of the aesthetic consciousness*[1]. Given the aesthetic point of view, nothing in life or nature or Art need be aesthetically ineffective, as in point of fact, nothing is entirely barren as a possible source of aesthetic delight.

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[1] Objects and experiences differ, of course, considerably in the ease with which they can be maintained on the aesthetic level. The statement holds only in theory. In practice there will always be instances in which it proves impossible. It is in practice for this reason that Art is superior to 'Nature' in aesthetic value and effectiveness.

CRITICAL NOTICE.

BY G. UDNY YULE.

The Essentials of Mental Measurement. By WILLIAM BROWN and GODFREY H. THOMSON. Cambridge, at the University Press. 1921. pp. 216. Price 21*s*. net.

This is a revised and expanded edition of Dr Brown's well-known little work, bearing the same title, which was published in 1911 and has now been for some time out of print. The format has been increased from demy to royal 8vo; the number of pages from 154 to 216, and the price (alas!) from 3*s*. 6*d*. to 21*s*. net. Dr Brown, owing to his time being taken up with Army medical work during the war, was unable to take any further part in the development of applications of statistics to psychology after 1914 and the revision of the work, we gather, is almost wholly due to Professor Thomson.

The additions and revisions are very considerable, and for the benefit of those who know the old edition it may be convenient to describe them. In *Part I. Psychophysics* two new statistical chapters have been interpolated. Chapter I on Mental Measurement is now followed by a chapter on "The Elementary Theory of Probability" which covers a very wide field—frequency distributions, averages and measures of dispersion, the standard error of the mean, the binomial distribution, and the normal curve. The former Chapter II on the psychophysical methods succeeds this, and then comes the second new chapter on "Skewness and Heterogeneity in Psychophysical Data," in which Professor Pearson's test of goodness of fit is described, his system of skew curves, and the analysis of a compound distribution into two normal curves. The former *Part II. Correlation* has been even more largely expanded. The Introduction and the chapter that followed on the Mathematical Theory of Correlation remain the same in title though the matter has been revised. Chapter VII, on the Influence of Selection, covers not only the effect of selection on standard deviations and correlations, but also multiple correlation, 'spurious' correlation ('spurious' is an unfortunate term which is better avoided) and a short account of the variate-difference method. In Chapter VIII the authors consider the correction of raw correlation coefficients for irrelevant factors and for observational errors, and in the two final chapters the theory of general ability and the hierarchy. There are some useful tables at the end of the book and excellent bibliographies.

The exposition, it may be said at once, is admirably lucid, as one would expect from both authors, though it is at times extremely compressed; the student will often have to refer to other volumes or to the original memoirs if he wishes adequately to study some of the methods of which hardly more than an outline is given. Certainly the volume is one which should be in the hands of every investigator who is using the methods considered. And yet—if I were asked whether the volume was entirely a satisfactory one I should feel bound to answer, No. My objection is not to the manner of exposition, but to the matter chosen. This may seem an unfair objection: an objection merely to the authors (for I take it that both authors must be held responsible for the matter included) having given us one sort of book when I would rather that they had given us another. But I do not think that my particular objection is unfair. A book, a good book, should be written with one aim, one outlook. The present book is heterogeneous: it is in part a text-book and in part a polemical pamphlet. If the reader skips the prefaces, to avoid the initial note of controversy, he will find himself reading an ordinary peaceful treatise until he gets to Chapter VI or perhaps Chapter VIII. In Chapter VI the trumpets begin to sound, though gently and musically, about the method of ranks. But with Chapter VIII, and the correction of correlation coefficients for errors of observation, the *mêlée* is fairly started and Chapters IX and X continue the fight on the field of the hierarchy. It should not have been impossible to write an exposition in a less controversial spirit. It would have been perfectly possible, for example, to exhibit the methods of correction of a correlation coefficient for uncorrelated errors without criticism of Professor Spearman's use of the formulae, to emphasise the assumptions made, to show what happened when errors were not uncorrelated, and to illustrate the inapplicability of the assumptions (or of certain of them) in the few cases that have been tested. It would have been perfectly possible, in the same way, to have shown how different systems of correlations arise from specific assumptions as to the nature of the causation—variables which are linear functions of general, specific and group factors; the dice-throwing cases on which Dr Thomson has made himself a specialist; and so on—and to have shown the student how difficult an aggregate of correlations might be to interpret, without entering into controversy at all. References at the end of the discussion to show the reader how questions of interpretation had arisen would have been quite sufficient. In spite of the controversy it is pleasant to note the statement by Dr Brown in his preface that as an opponent he has learnt to admire the work of Professor Spearman to a very great degree, and regards his work in correlational psychology as epoch-making in its significance.

As briefly as possible, a few comments may be made on the points of controversy. First as regards the method of ranks, that is of ranging individuals in order as regards any quality, numbering them off, and taking the number assigned to each individual as the measure of his quality for purposes of correlation. The alternative is to assume that if the two qualities correlated could be measured the correlation between them would be normal, and to use an approximate formula based on this assumption. Dr Thomson summarises his views on this point most clearly in his preface. He makes the, possibly very valuable, suggestion that it may be practicable to approximate to the true distribution by estimating not only ranks, but the relative order of differences between the ranked individuals, of the differences between these and so on. Failing this, "the question is whether it is more probable that the assumed or the measured distribution corresponds to the actual unknown mental distribution" (p. vii). On this I would make two comments. In the first place, as it seems to me, the mental distribution is not merely unknown but totally indeterminate and it is impossible to speak of an 'actual' distribution, or of two intervals on the scale being equal or unequal, until the scale has been agreed on. Secondly, it is always possible to choose such a scale that the distribution of any variable taken singly shall be normal—though of course a scale so fixed is arbitrary and may be unnatural: but the assumption made in using Professor Pearson's formula for obtaining the 'true' correlation from the correlation for ranks is one that it is *not* necessarily possible to secure by any choice of scale. It is assumed not only that the distribution for each variable taken singly is normal, but also that *the correlation is normal.* A choice of scales such that the distribution for both variables is normal will only make the correlation normal, if it was originally of the type that I have elsewhere termed 'strained normal[1].' Given sufficient observations the existence of this type of distribution can perfectly well be tested in either of two ways. (1) A contingency table can be drawn up for the ranks, using at least three rows and three columns, and by the formulae of pp. 124–5 it can be seen whether this can be regarded as a normal distribution with the assigned correlation. (2) Each rank can be replaced by the equivalent normal deviation and the correlation then worked out between these deviations. As the validity of the assumption can be tested, there is no justification for using it without the test. It may be added that an objection may be brought against the use of any approximation which assumes normality, precisely of the same kind as is brought against one of Professor Spearman's methods by Dr Thomson—namely that, even if the assumption may be nearly true for a large aggregate of observations, it cannot be true for small samples. The method (2) suggested above is always applicable and may, perhaps, be of service—but it is not so brief as the pure ranks method. Given, however, the same number of observations, the standard deviations can be determined once for all.

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[1] Cf. my paper on Association in the *Journal of the Royal Statistical Society*, May 1912, Sections 39 et seq.

On the question of correction of correlations for errors of observation, I would like to point out that the formulae for correction only assume that errors and quantities measured are uncorrelated—that the correlation coefficient is zero—not that they are strictly independent, and the fact that the *correlation-ratio* in any given case is large is of no importance at all: the correlation-ratio on p. 160 ought not to be cited in this connection. In that case the correlation *coefficient* between errors and variables is practically zero: that is all that is required, and surely, in spite of the authors' scepticism, what ought to be expected? No experimenting would be of any value at all if there were a liability to biased errors, which could not be eliminated by averaging, at different points of the scale. The correlation between simultaneous errors in the observed magnitudes of two variables is another matter; but here again to allege that such correlation cannot be eliminated by proper methods of experimenting seems to me to be taking a very hopeless attitude. Moreover I cannot help thinking that some blunder has exaggerated the evidence as to the existence of the latter type of correlation. I must decline to believe that with 43 observations the probable error of a product-sum can be of the order of one-half of one per cent. of the quantity dealt with, as alleged by the figures on p. 159: and I note that the formula given, which I do not recognise, is of the wrong order in n (the number of observations). On the general principle I agree, however, with the authors that no formulae assuming lack of correlation can be trusted for small samples and that "it is an essential of good work to use such samples that corrections to the raw values obtained are unimportant" (p. 161).

Now as regards the 'hierarchy' controversy, let me first take the points with which I find myself in agreement with the authors. I agree that the Hart-Spearman criterion is unsatisfactory, and I agree that the use of the 'correction' formulae has been dangerous and possibly misleading. But both these conclusions leave me rather cold, for in the very little work that I have personally done on the hierarchy question I used Burt's uncorrected coefficients and did not employ the criterion, and the results left no doubt at all in my own mind that the assumption of a general and of specific factors gave an extraordinarily good fit over the part of the table that I tested. Where Dr Thomson's work seems to me most important is in his proof that quite other assumptions may lead to true hierarchies, and yet other assumptions again to distributions which may in some degree ape the form of the hierarchy and, even with a sufficient number of observations, be mistaken for that form under careless or incorrect methods of examination. But I part company altogether with him in his view that hierarchical order "is the natural relationship among these coefficients, on any theory whatever of the cause of the correlations, excepting only theories specially designed to prevent its occurrence. It is the *absence* of hierarchical order which would be a remarkable phenomenon requiring special explanation; its presence requires none beyond what is termed chance" (pp. 183–4). Let us be clear, in the first place, as to what is meant by hierarchical arrangement: its accepted meaning—I think I am right in saying—is that

$$\frac{r_{km}}{r_{kn}} = \frac{r_{sm}}{r_{sn}}$$

for all values of k, m, n, and s. Dr Thomson seems at times to degrade the meaning of the phrase to imply no more than positive correlation between r_{km} and r_{sm} in any pair of columns. I use the phrase in the first sense, and cannot regard any table as hierarchical unless the rule holds—strictly for theoretical coefficients, or within the limits of probable error for observed coefficients. Now it is not necessarily true that "coefficients of correlation are themselves correlated" if by this is meant that r_{km} and r_{sm} are positively correlated for any pair of columns k and s. Fluctuations of sampling in the r's are so correlated and this may give rise to some trouble in interpreting observed tables, but not necessarily to a hierarchy in the strict sense. Nor—so far as I can see—

can 'chance' give rise to a hierarchy. Dr Thomson's mechanism of pp. 175–7 in the first place does not give rise to a hierarchy at all, but only to positive correlation between columns, and in the second place surely cannot be termed a 'chance' mechanism—all the important conditions were predetermined by Dr Thomson himself[1]. The result can no more be termed a 'chance' result than a cubical parabola can be termed a chance law if the values of the coefficients are determined by drawing cards from a pack. As a matter of experience, I may say, no one of the few cases that I have examined from non-psychological fields yields a hierarchical arrangement.

It seems to me impossible to agree then that the hierarchical arrangement found by some observers is a 'chance' result or does not call for explanation. The difficulty is that there may be more than one explanation. From the statistical standpoint Dr Spearman's explanation seems to me to be by far the simplest, but the judgment as to its validity must be based on other grounds.

It is not, or certainly not wholly, the fault of the authors, but the reading of this volume has produced in my own mind a feeling of extreme depression. "The Essentials of Mental Measurement": a fine, rhythmic, magniloquent title! A heartening picture it calls up too: *Essentials*... the real root of the matter here! *Mental*... we are really going to get at the *mind* now, look you! *Measurement*... absence of personal bias... precision...splendid! But what do we find? *Essentials*—a few experimental methods, rather difficult to interpret: a mass of statistical methods, which ought to be termed ancillary, not essential: and a great deal of controversy. *Mental*? well it is no good attempting to pretend that many of the data (or *any* of the data?) can be termed mental. Even the authors, with their title before them, can only hope that their arguments have sufficed to "show that purely psychical measurement is a *conceivable possibility*" (p. 9, my italics) and that their data are "indirect psychical measurements" (p. 10). And *measurement*! O dear! isn't it almost an insult to the word to term some of these numerical data *measurements*? They are of the nature of estimates, most of them, and outrageously bad estimates often at that. I have begun to visualise the title of this book as "The Essentials (?) of Mental (?) Measurement (?)."

Statistical methods, I say, should be regarded as ancillary, not essential. They are only essential where the subject of investigation is itself an aggregate, as a swarm of atoms, or a crowd. But here the subject is the individual, not the aggregate of individuals as such. This being the case, statistical methods are only necessary in so far as experiment fails to attain its ideal, the ideal of only permitting one causal circumstance to vary at a time. And it should always be the aim of the experimenter not to revel in statistical methods (when he does revel and not swear) but steadily to diminish, by continual improvement of his experimental methods, the necessity for their use and the influence they have on his conclusions. Statistical methods are not only ancillary; they are, to the experimenter, a warning of failure.

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[1] Compare Dr Garnett's article in this *Journal*, x. 242.

During the last year or two I have made it a practice to begin a short course of lectures to agricultural students, on some elements of statistical methods, by a lecture devoted entirely to showing how exceedingly bad agricultural experiments are, how this introduces the necessity for statistical methods in discussing the results, but does not obviate the primary necessity for improving the experimental methods. Is the latter warning unnecessary in the case of experimental psychology? I can only speak as a statistician with an exceedingly limited and sporadic knowledge of what is being done, but it seems to me that it is urgently necessary. The experiment (if it can be called an experiment at all) is often not only bad, but its badness seems to be accepted merely as a rather interesting fact, calling perhaps for the exercise of more elaborate statistical methods—a few partial coefficients of correlation—a few rather elaborate probable errors—and that is all. It does not seem to be recognised as primarily a condemnation of the experimenter, perhaps because the experimenter has never been exercised in the sciences where really accurate experiment is possible, so that he has no high ideal before him. Let me cite again from the volume before me one instance that I have mentioned already. On p. 160 it is seriously suggested that we cannot safely assume the error of measurement to be uncorrelated with the quantity measured. It does not seem to have occurred to the authors that if the suggestion were true (it was made perhaps under the domination of the Spearman-complex) it would be an utter condemnation of the science on which they were writing. Unless it were possible to assume a great deal more, namely that the average error was zero for all values of the quantity measured, it would hardly be legitimate to speak of the data as measurements at all: their value for any useful purpose would be nil.

The New Psychology that *was* the new psychology some five-and-twenty years ago has had to surrender the title to a yet newer successor. What has happened in the interval? Measurement does not necessarily mean progress. Failing the possibility of your measuring that which you desire, the lust for measurement may, for example, merely result in your measuring something else—and perhaps forgetting the difference —or in your ignoring some things merely because they cannot be measured. *Has* measurement in this field led to progress? The impression left by "The Essentials of Mental Measurement" is not cheering on the point.

PUBLICATIONS RECENTLY RECEIVED

The Letters of William James. Edited by his son, HENRY JAMES. London: Longmans Green and Co. 1920. Vol. I, pp. xx + 348. Vol. II, pp. xiii + 382. 42*s*. net.

These letters, admirably edited, are all of an intimate nature, revealing with wonderful clearness the attractive character of their writer—his general enthusiasm, his readiness to praise, his love of philosophy, his devotion to work and duty despite numerous interruptions owing to eye-strain, insomnia and fatigue. In them will be found little concerning the development and nature of his views on philosophical or psychological problems. When a medical student, only 21 years old, he wrote that of all departments of Medicine, that to which Dr Prince devotes himself [Psychiatry] is, he would think, the most interesting (I. 44). Writing in 1867 from Berlin, aged 25, he says, "it seems to me that perhaps the time has come for psychology to begin to be a science" (I. 118). Nine year's later he "organised the beginnings of the psychological laboratory" (I. 179) at Harvard, and two years after that—during his honeymoon—he began on his *Principles of Psychology*. Of this he wrote, before its publication in 1890, "I have to forge every sentence in the teeth of irreducible and stubborn facts. It is like walking through the densest brush-wood" (I. 225). "I have written every page four or five times....If there is aught of good in the style, it is the result of ceaseless toil in re-writing." After the publication of the *Principles*, James's main interests veered to philosophy. "My experimental aptitude," he had confessed to Stumpf (I. 249), "is but small"; and now (in 1896, aet. 54) he writes to Flourney, "*I* have got rid of the laboratory for ever....The results that come from all this laboratory work seem to me to grow more and more disappointing and trivial. What is most needed is new ideas. For every man who has one of them one may find a hundred who are willing to drudge patiently at some unimportant experiment" (II. 54).

Psychology and the Day's Work. By Professor E. J. SWIFT. New York: Charles Scribner's Sons. 1918. Pp. ix + 388. $2.00 net.

A review of this ably written and interesting book is impossible, so wide is the field which it covers. The expert psychologist will find in it little to criticize and little that is new beyond the illustrations, abundantly quoted by the author from relatively unfamiliar sources. The ten chapters deal respectively with organization for mental efficiency, thinking and acting, habit in preparation for efficiency, the psychology of learning, fatigue and its psychology, curiosities of memory, memory and its improvement, the psychology of testimony and rumour, our varying selves, and the psychology of digestion. Thus, as he explains, "the writer has tried to select types of conduct, as well as phases and causes of behavior, that are fundamental to thinking and acting, whether in the life of social intercourse or in the business and professional world." To the general public engaged in these spheres of intercourse the book may be confidently recommended.

A Young Girl's Diary. Prefaced with a letter by SIGMUND FREUD. Translated from the German by EDEN and CEDAR PAUL. London: Allen and Unwin. 1921. Pp. 272. 12*s*. 6*d*. net.

This book, for obvious reasons specifically addressed to doctors, parents, teachers and students of psycho-analysis, purports (without adequate evidence) to be a diary written by a young Austrian girl of the upper middle class between her twelfth and fifteenth year. It will appeal strongly to those convinced of the importance of sexual thoughts in early life.

In Search of the Soul and the Mechanism of Thought, Emotion and Conduct. By Dr BERNARD HOLLANDER. London: Kegan Paul. N.D. Vol. I, pp. x + 516. Vol. II, pp. vii + 361. 42*s*. net.

The first of these two massive volumes is historical, and finally leads to a vigorous defence of Gall and of phrenology. The second deals with 'ethology' (the science of character), the mental functions of the brain, genius, insanity and crime, and the "unexplored powers of the mind." The following quotations serve to illustrate the writer's qualifications and standpoint: "The greater the strength and activity of a mental quality, the greater the pleasure attending the exercise" (II. 5). "It has been shown in the previous chapter that the pleasurable passions are connected with the front part of the brain. In this chapter it will be shown that the depressing emotions are connected with the posterior part of the brain" (II. 123). Dr Hollander seeks to relate melancholia to the functions of the parietal lobe, mania to those of the base of the temporal lobe, "active sexual propensity" to "a large nape of the neck," etc. He gives an elaborate account of the now completely discredited N rays, and he "refers to the probable existence of a human aura" (II. 301), and to the "undoubted communication...both in the waking state and the hypnotic state between mind and mind otherwise than through the known channels of sense" (II. 312).

A Study of Women Delinquents in New York State. By M. R. FERNALD, M. H. S. HAYES, A. DAWLEY and others. New York: The Century Co. 1920. Pp. xviii + 542.

"The purpose of the present study," published by the Bureau of Social Hygiene, "may be stated as an attempt to furnish a scientific basis for the conceptions regarding women offenders, through an investigation concerning the distinguishing characteristics of women convicted of either serious crimes or minor offenses in New York State" (p. 5). The first comparison attempted was between "one group of women delinquents and women in general"; the second was between the women offenders and "similar groups of men offenders." Unfortunately, reliable data were often lacking with which those obtained from the women offenders could be compared. Consequently, as the authors state, they are compelled to "offer data on one side of the comparison without [always] being able to secure the complementary information on the group to be compared" (p. 8). Thirdly, they divided the women offenders of the group studied into various classes, according to duration of delinquency, age at first conviction, drug habits, physical data, birth-place, colour, family life, educational opportunities, mental capacity, occupational history, sex irregularity, etc.; and they attempted to study the distinguishing characteristics with varying methods of classification. The statistical treatment appears to have been well carried out. The psychological examination was conducted on about 500 women, based on the Yerkes-Bridges Point Scale, the Stanford-Binet, the Woolley and other tests. The social investigation was so planned as "to obtain from each woman delinquent studied an oral statement giving the details of her personal history and her background" (p. 61), the information thus given being verified by other data and inquiries carefully recorded, and the tendency, if any, being noted towards misrepresentation or reticence. The authors' general conclusion is that far too little importance has been of late attached to 'nurture,' excessive stress being laid on 'nature.' They believe that 'environment' is shown by their data to be a more potent cause of delinquency than 'intellectual ability.' But they do not consider whether other defects, of affective or conative character, are correlated with crime.

Religion and the New Psychology: a psycho-analytic Study of Religion. By W. S. SWISHER. London: Routledge and Sons. N.D. Pp. xv + 261. 10*s*. 6*d*. net.

"There is an increasing body of literature," the author announces in his foreword, "which deals with the new psychology and the psycho-analytic principle." This 'body' is increasing at such a rate as to lack substance and significance. A less superficial and more judicious examination of the psychological bases of religion, with a really critical application thereto of the psycho-analytical standpoint, is greatly needed. But Mr Swisher's book is far from meeting this need.

Mysticism, Freudianism and Scientific Psychology. By Professor KNIGHT DUNLAP. St Louis: L. V. Mosby Co. 1920. Pp. 173. $1.50.

Prof. Dunlap argues that there is at the present day a dangerous increase of mysticism, especially of what he calls 'quasi-' and 'pseudo-mysticism,' which includes "telepathy, clairvoyance and other supernatural methods of acquiring information or knowledge." Then he points out (p. 40) that mystical expressions are closely related "to physiological conditions of a distinctly sexual nature" and "that the description of ecstasy in many cases would pass equally well as a description of the sexual organism." Finally, he describes psycho-analysis as "one of the most important if not the most important mystical movement of the nineteenth century" (p. 44). He holds that "...psycho-analysis is essentially antagonistic to scientific psychology," that "in the Freudian system appears the fundamental anti-scientific postulate of mysticism: a form of knowledge—consciousness—which yet is not consciousness....To this mystic knowledge...is ascribed an importance far above that of consciousness itself" (p. 89). The unconscious mind, then, is, for Prof. Dunlap, 'mystical' and 'anti-scientific.' He holds that it "is almost the exact correspondent of the philosophical mystic's third kind of knowledge" (p. 46), additional to the knowledge obtained by perception and inference. "The whole poisonous vine [of psycho-analysis], with its tendrils threatening to grasp and choke all forms of learning...is rooted in the assumption of an 'unconscious' which is not merely the nervous system, and not merely something which isn't conscious at all, but is truly a mystical *third kind of knowledge*" (pp. 162, 163). Comment is superfluous. Although such hostile prejudice and misunderstanding make just, reasonable criticism on the part of the author impossible, nevertheless illustrations of various absurdities and rash conclusions adopted by psycho-analysts occur scattered throughout the book.

Standard Method of Testing Juvenile Mentality. By N. J. MELVILLE. Second enlarged edition. Philadelphia and London: J. B. Lippincott Co. 1920. Pp. vii + 162. 12*s*. 6*d*. net.

This book describes the general procedure to be adopted in gathering and analysing mental tests, and gives a standard method of applying the Binet-Simon scale, with note of the various modifications adopted by Goddard, Yerkes, Stanford, etc. The appendix includes tests for persons above fifteen, instructions for giving the Porteus maze tests, etc.

Die Intelligenz der Kinder und Jugendlichen und die Methoden ihrer Untersuchung. Von Dr WILLIAM STERN. Leipzig: J. A. Barth. 1920. S. xi + 335. M. 48.

This is issued in place of a third edition of the author's *Intelligenzprüfungen an Kindern und Jugendlichen.* It is divided into four sections, of which the first deals with the nature, varieties and development of intelligence, the second with the tests of the separate functions of intelligence and with its general measurement, the third and fourth with the applications of such tests to defective children and for the selection of those best fitted to proceed to higher education.

Einführung in die Metaphysik auf Grundlage der Erfahrung. Von Professor G. HEYMANS. Leipzig: J. A. Barth. 1921. S. vi + 364. M. 56; geb. M. 61.

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AFFECT IN THE DREAM

BY W. H. R. RIVERS

Affect in the dream and the concept of censorship. The nightmare as a failure of wish-fulfilment; character of affect due to this failure; partial failure and degrees of unpleasant affect. Infantile character of nightmare. Influence on affect of mental level action in the dream

IN an earlier paper[1] I have suggested an alternative explanation of certain features of the dream which Freud explains by means of his concept of the censorship. I have supposed that the fantastic character of the dream is not due to any process of distortion designed to elude the vigilance of some anthropomorphic agency watching at the threshold of consciousness, but is the natural result of the infantile character of the dream. I regard this character as the direct consequence of the coming into activity of modes of behaviour which in the waking state are held in check by levels of mental activity embodying the experience of later life.

It is the aim of this paper to show that the affective character of the dream which Freud has referred to the activity of the censorship is also the natural result of the character of the dream as an expression of infantile mentality. According to Freud one character of the dream is that its manifest content is as a rule poorer in affect than the dream-thoughts. Where there is an affect in the manifest dream it will also be found in the latent content, but the converse is not true. There may be no appreciable affective disturbance in the manifest dream when the presence of affect in the deeper content is evident. In other words, one of the results of the transformation of the latent into the manifest content of the dream is not only to disguise the nature of the dream-thoughts from the sleeper, but also to lessen or inhibit its affective character; and just as Freud ascribes the fact of disguise to the censorship, so does he ascribe to this agency the lessening or inhibition of affect. I will begin by saying that my own experience definitely confirms Freud's statement that affect may be absent or at least inappreciable in the manifest dream when it is evidently present in the deeper dream-thoughts of which the manifest dream is the transformed expression. This is a definite fact which has to be explained by any theory which endeavours to account for the relation of the dream to the affective aspect of experience. I propose, however, to begin the consideration of this subject by dealing with a variety of the dream in which affect is not merely present in the manifest content, but is present in an extreme degree. In the nightmare there is painful affect of the most intense kind, and any theory of the dream must take account of this character. This variety of dream, in the form of so-called night-terror, is especially liable to occur in childhood, but examples of one kind or another often occur in adult life, especially under abnormal circumstances. Everyone who had to do with war-neurosis became very familiar with this form of dream as a characteristic example of the nightmare.

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[1] "Freud's Concept of the 'Censorship,'" *Psychoanalytic Rev.* 1920, VII, 213; republished in *Instinct and the Unconscious*, 1920, p. 228.

The nightmare of war-neurosis generally occurred at first as a faithful reproduction of some scene of warfare, usually some experience of a particularly horrible kind or some dangerous event, such as a crash from an aeroplane. A characteristic feature of this variety of dream is that it is accompanied by an affect of a peculiarly intense kind, often with a special quality described as different from any known in waking life. The dream ends suddenly by the patient waking in a state of acute terror directly continuous with the terror of the dream and with all the physical accompaniments of extreme fear, such as profuse sweating, shaking, and violent beating of the heart. Often the dream recurs in exactly the same form night after night, and even several times in one night, and a sufferer will often keep himself from sleeping again after one experience from dread of its repetition.

Two of these features are of special interest in relation to the place of affect in the dream. One is that the affective disturbance is extreme in amount; the other, that the dream is often the repetition of an actual experience without transformation of any kind. There is absolutely nothing of the grotesque or fantastic, but the dream follows the grim reality faithfully. Moreover, it is often in my experience one of the first signs of improvement that some amount of transformation appears; the events of the actual experience are replaced in the dream by incidents of other kinds, such as the appearance of terrifying animals, which stand in no direct relation to the actual war-experience of the dreamer. Though in these cases the dream continues to be accompanied by fear, this is less intense and accompanied by less severe physical manifestations, and in many cases this transformed character serves as a stage towards the disappearance of the 'nightmare' character of the dreams. The course of many of these cases suggests that there is a definite relation between the amount of transformation and the intensity of the affect. They suggest that the intensity of affect is inversely proportional to the amount of transformation, a suggestion in harmony with the view of Freud that one of the results of the transformation of the latent into the manifest content of the dream is to lessen or inhibit its affective character.

When, however, we examine these dreams more closely we find that they show features which can hardly be reconciled with the general Freudian position. Above all is the difficulty of reconciliation with the view that every dream is a wish-fulfilment. It is difficult to see how such awful and terrifying experiences as those of dreams of this kind can be the result of wishes of the dreamer. Even if there were no other facts to lead us to regard Freud's view that the dream is a wish-fulfilment as unduly simple, and in my opinion there are many such facts, the nightmare and the battle-dream would themselves be sufficient to lead us to revise the Freudian view. In its place I am accustomed to look upon the dream as the attempted solution of a conflict, an attempt to solve a conflict of the waking life by such means as still remain open when the higher levels of mental activity have been put out of action by the inhibition of sleep.

Following this line of thought let us inquire how far the nightmare and the battle-dream are capable of explanation as infantile attempts to solve a conflict. In considering this matter I will begin with a feature of dreams of this kind which I have not so far mentioned.

In most, if not all, battle-dreams it is found that in the waking state the dreamer has been striving to keep out of his consciousness the experience which is reproduced in the dream. He has been repressing this experience. It has been found over and over again that when this process of repression is given up, the dreams no longer occur, or, if they continue, lose their terrifying character. A large body of evidence accumulated during the later years of the war showing that these terrifying dreams are due to repression of the experience which forms the content of the dream. So far as desire enters into causation, the dream is the direct negation of a wish, the wish not to be subjected to the repetition of a painful experience, the wish leading to a process of repression in the waking life which in its turn produces the dream in sleep.

Taking the process as a whole it seems to have the following character. One who has dreams of this kind has been the subject of a painful experience which tends to obtrude itself upon his attention, a tendency which is counteracted by a process of repression, a process of keeping the experience out of attention whenever it tends to appear. The conflict is one between a process in which an experience tends to recur to memory and a desire that the experience shall not recur. So long as the subject of this conflict is awake, the process of repression has the upper hand, but as soon as sleep occurs and the process of witting repression is removed, the repressed experience meets with no obstacle and makes its presence felt with full force. From this point of view the dream, instead of being the fulfilment of a wish, conscious or unconscious, is the complete failure of a wish which is only effectual so long as the subject of the experience remains awake. The experience of the nightmare not only fails wholly to fit into the category of wish-fulfilment; it suggests that this form of dream is essentially an expression of the complete negation of a wish.

If now we turn to consider how far a dream of this kind can be regarded as the solution of a conflict, we find that a conflict is undoubtedly present, viz. a conflict between the tendency of an experience to recur and the wish that it shall not do so. But if the dream is regarded as a solution of this conflict, it is a solution of a quite unsuccessful kind, so far as the health and happiness of the dreamer is concerned. From this point of view a dream of this kind must be regarded as a failure of solution, and, if I am right in my view concerning the dependence of this form of dream upon repression, a failure directly due to a wrong method of meeting the situation. The whole process belongs to a category of a kind very different from that in which we place dreams of the more usual kind.

In the case, on the other hand, in which the dream begins to show a certain amount of transformation with the accompaniment of less intense affect, we find ourselves in the presence of a process of the same order as that with which we are acquainted in dreams of the customary kind. It is only when the process of transformation begins that it becomes at all possible to consider the nightmare as the solution of a conflict, and since in this case the chief result at first is to lessen the intensity of the painful affect, it can only be regarded as a solution of an imperfect kind, the effect of the transformation being to lessen to some extent the painfulness of an experience which the dreamer wishes to keep altogether out of his consciousness. If we include the nightmare in the dream-category, it becomes no longer possible simply to regard the dream as the solution of a conflict, though it may still be possible dreams of my patient was due to the dream-consciousness having reached a solution contrary to the desires most prominent and most potent in the sleeper's consciousness when awake.

Having reached this solution of our problem, I can now consider another aspect of the dream so far as its affective nature is concerned. According to the view here put forward, dreams are not only regarded as solutions of conflicts, but as solutions of a kind differing from those adopted in healthy waking life in that the solution has an infantile character[1], or at least a character belonging to a period of life earlier than that of the occurrence of the dream. Let us now inquire how far this infantile character is true of the nightmare, again taking the war-dream as our instance. The first point to notice is that under the ordinary circumstances of our modern civilised life, dreams of this character are exceptional in the adult. It is in childhood that they are best known, forming the basis of the night-terrors from which so many children suffer. When they occur in the adult, they may be regarded as examples of regression to a state characteristic of childhood.

A feature of the war-dream, and of other adult nightmares, is the exaggerated character of the fear experienced, this exaggeration being again a character of infancy and childhood. There is little doubt that infancy and childhood form a period of life in which the human being is liable to affective disturbances of a very intense kind with the crude explosive nature which is characteristic of the affect of the nightmare or war-dream.

Still another feature of the war-dream, and probably also of the adult nightmare in general, is of special significance. The affective disturbance is described as having a peculiar quality unlike any experience of adult waking life, [illegible] ralleled by the gusts of un- [illegible] lly to overwhelm sufferers [illegible] of course possible to the incidents of warfare which have hitherto formed the exclusive content of the dream. There is little question that animals are prominent in the terrifying dreams of childhood, and their occurrence in the transformed war-dream may thus be regarded as another example of regression. Some of my patients remembered having had similar dreams in childhood, while in one case in which the terrifying image was a Chinaman, the dreamer distinctly recollected its occurrence in the dreams of his childhood. The images utilised in these examples of transformation were characteristic of an early period of life.

The examination of the nightmare and war-dream thus shows that they possess, though in a different way, a character which I have ascribed to dreams of the more ordinary kind. The nightmare and the war-dream share with dreams of other kinds the feature that they are occurrences in which experience finds expression in sleep in a form characteristic of infancy or of periods of life earlier than that of the occurrence of the dream. While it is not possible to regard all dreams either as wish-fulfilments or as successful solutions of conflicts, it is possible to bring them all into the category of regression, of throwing back in sleep to modes of mental activity and expression characteristic of earlier periods of life.

While the pure battle-dream thus differs from the general run of dreams in respect of transformation, it falls into line with the rest with regard to the infantile form in which the affect finds expression. The infantile character gives a broader basis for classification than is afforded by the process of transformation. This being so, let us inquire how far this infantile character is capable of explaining, or at least of illustrating, the relation of the affect to the dream. For this purpose I shall use a dream which Freud has cit[illegible] order to illustrate this topic. In this dream Freud cleanse[illegible] by the same means as[illegible] other hand, his own rôle as the Hercules of the occasion seemed to Freud to correspond with certain grandiose traits he detected in his own attitude towards the situation. Freud refers the absence of disgust in the dream to the fact revealed by his analysis that the thoughts of cleansing an Augean stable were combined with others of a pleasant kind referring to his own powers as a cleanser. The indifferent character of the dream in its affective aspect is referred to the combination of the two opposed kinds of affect present in the dream-thoughts.

On the view that the dream is an expression of infantile modes of mental activity, another and a simpler explanation of the absence of disgust becomes possible. From this point of view Freud's dream is an expression of a conflict between his desire to get away from certain disgusting aspects of his professional work and the desire for the beauties and pleasures of a holiday. The dream produces a symbolic solution of the conflict by getting rid of the disgusting aspects of his work by means of a procedure which would make a direct appeal to the mind of a child and is in consonance with a child's modes of activity. The absence of disgust in the dream seems to receive an explanation more natural and at the same time more simple than that given by Freud if in his dream he had for the time regressed to an infantile attitude and was seeking to purify his surroundings symbolically by a procedure characteristic of childhood. In the child such a procedure would not arouse the emotion of disgust and therefore no such emotion was aroused in the dream. The indifferent affective character of this dream becomes natural and needs no elaborate explanation if the dream be an infantile mode of expression of a wish-fulfilment or the solution of a conflict.

This dream of Freud's is [illegible] which the absence of affect in a [illegible] occur is readily accounted for [illegible] turn of which the dream-homicide was a symbolic expression. In this case the difference lies not so much in the relation between affect and the incident which called it forth, as in the nature of the affect itself. The infantile character shows itself in the intense and explosive, or as I have expressed it elsewhere[1], in the 'all-or-none' character of the affect, while in the real situation this would have had the more restrained form which emotion normally assumes in adult life.

Again, if the transference dream was an expression of infantile modes of feeling and acting, we have a case in which we may suppose that the child of the dream was being forced to turn away from one upon whom he had come to rely, and if current views concerning transference have any value, from one whom he had put in the position of his father. The intense affect of the dream, inappropriate perhaps to the actual situation of the dream, certainly inappropriate to the situation of having to turn from the help given to him by his physician, becomes wholly appropriate if in the dream the man had become again the child and was turning away from his father and ceasing to depend upon him for help and comfort[2]. The intensity of the affect becomes intelligible if the dream be a regression to infancy, just as the absence of affect becomes intelligible if the dream-situation is one which the infantile mind would regard with indifference.

I have so far dealt almost entirely with painful or unpleasant affect in its relation to the dream. It is now necessary to say something about the affects of a pleasant kind which so often accompany the dream. We have been led to connect the degree of intensity of painful affect present in the dream with the extent of the process of transformation, the absence of affect being correlated with the completeness of this process. I have supposed that the presence, nature and amount of affect present in the dream stand in no defin[illegible]
amount of affect accompany[illegible]

symbolism in which a conflict is finding expression are of a kind so natural to the child that they would not be accompanied by affect, there will be no affect in the dream. The situation as portrayed in the dream may be one of adult life, but the dream-reaction is that of the child so far as affect is concerned.

If we now apply this point of view to the case in which the affect of a dream is of a pleasant kind, it will again be necessary to distinguish between dreams with varying degrees of transformation. We should expect pleasurable, as painful, affect to be the more definite, the less the degree of transformation. Where there is no transformation, we should expect the affective accompaniment to be considerable in amount. As an example of this may be cited the dreams of the kind, to which I have already referred incidentally, where the manifest content has a sexual nature. Here the pleasant nature of the affect is just as natural to the situation as the fear or terror accompanying a dream of which the manifest content has reference to danger.

Another variety of dream accompanied by affect of a pleasant kind is that in which the manifest content has the nature of a day-dream. In this case again there is little if any transformation, but there is carried over into the dream an experience of waking life which in that waking life is definitely accompanied by pleasant affect. It may be suggested that the occurrence of the experience in sleep gives it that enhanced vividness and apparent reality which is characteristic of the day-dreams of childhood, and that the pleasantness of the affect is associated with this approach to an infantile character.

At the beginning of this paper I mentioned a view of Freud to which I may now return for a moment. According to [illegible] the lessening or disappearance of affect which accompani[illegible] [illegible]ent into the [illegible] no agency watching at the threshold of consciousness for their explanation. Freud's concept of the censorship was especially devised to meet the cases where affect is lessened or abolished and fails wholly to account for such dreams as those of my patient. The explanation of the nature of the affect in the dream as the necessary result of its infantile character not only renders the concept of the censorship unnecessary when it might seem to be appropriate, but it covers a wider field and is able to bring within its scope features of the dream with which this concept is wholly inadequate to deal.

I will conclude this paper by stating the conclusions reached in it. I start from the position that dreams are attempts to solve in sleep conflicts of the waking life, and that these attempts are necessarily, from the nature of sleep, of a more or less infantile kind, since in sleep only the earlier levels of mental functioning are active. Next, I suppose, and here I follow Freud or at least agree with him, that the effect of transformation is to diminish or abolish the affective aspect of the conflict. Consequently, when there is no transformation, there is affect in the dream. This affect is painful when the conflict fails to satisfy the most prominent wishes of the dreamer. On the other hand, it is pleasant when these wishes are gratified. But in the majority of dreams the affective aspect is slight or absent because the struggle is transformed and the solution of the conflict is only of a symbolic kind.

Moreover, just as I have previously argued that the transformation, ascribed by Freud to the need to evade the agency he calls the censorship, is a natural and indeed inevitable result of the expression of the conflict in the mental imagery and modes of mental functioning of early life, so do I now [illegible] the affect has a similar infantile quality, and [illegible] to have recourse to the agency of the [illegible]

THE LIMIT OF THE GROWTH OF INTELLIGENCE

BY P. B. BALLARD.

1. *The problem* (p. 125).
2. *Difficulties in the way of a complete solution of the problem* (p 126).
3. *The detection of absurdities as a test of intelligence* (pp. 126–128).
4. *The tests used* (pp. 128–131).
5. *Method of marking* (pp. 131–134).
6. *Results* (pp. 134–136).
7. *The curve of growth of intelligence* (pp. 136–138).
8. *The further use of the tests* (p. 138).
9. *The nature of intelligence* (pp. 138–141).

1. THE PROBLEM.

I begin by assuming that the term 'Intelligence' has an accepted meaning. Deferring a closer definition to the last section I meanwhile use the term as meaning that form of mental ability, whether simple or composite, which is measured by modern mental tests. To the plain man the most incredible part of this modern concept of intelligence is the early age at which intelligence is supposed to stop growing. Binet's scale of mental ages did not extend beyond fifteen. It is true that he added a group of five tests for adults, but these tests have since been found to be too difficult for the average adult. They fail to fit in with the rest of Binet's scale, which is strictly based on age-performance experimentally determined. Instead of indicating a norm they fix an arbitrary standard. As, however, Binet was primarily concerned with the detection of feeble-minded children it was but natural that he should have standardised the lower part of his scale more carefully than the upper part.

In making the Stanford Revision of the Binet Scale Terman[1] was influenced by no such restriction of purpose, and yet he could carry his age scale no further than fourteen. To represent the higher ages he has two sets of tests, one for the "average adult" and the other for the "superior adult." In calculating the Intelligence Quotient (the ratio the mental age bears to the chronological age) he assumes the chronological age of all persons over sixteen to be sixteen; which is equivalent to assuming that after sixteen years of age there is no improvement in native intelligence.

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[1] *The Measurement of Intelligence*, 56–61.

Burt[1] does not carry his scale of reasoning tests beyond fourteen.

The question to be determined is: At what age does intelligence cease to grow? As there are grounds for believing that this terminal age varies with the individual—that there is not one terminal age but many terminal ages—it is desirable to know both the average and the range of these terminal ages.

2. DIFFICULTIES IN THE WAY OF A COMPLETE SOLUTION OF THE PROBLEM.

The main obstacle in the way of research is the difficulty of securing unselected subjects over fourteen years of age. Up to fourteen years of age there is but little difficulty; for all children between five and fourteen are compelled by law to attend school, where they can be found and tested. But after fourteen in almost every type of school the duller pupils begin to leave; and as the ages increase the age-groups get increasingly smaller, and the quality of intellect increasingly higher. Such improvement therefore as one would find in the intelligence of secondary school children as they proceed up the school is not necessarily due to the growth of intelligence with age: it may be due to the gradual elimination of the less intelligent.

It is almost impossible to feel certain that one has secured a good representative sample of any age-group beyond that which falls between 13 and 14; and the further the group is removed from this the more difficult becomes the task.

3. THE DETECTION OF ABSURDITIES AS A TEST OF INTELLIGENCE.

Binet seems to have been the first to employ absurd statements as a test of intelligence. He used the following five absurdities, which be allotted to the eleventh year in the 1908 scale, and to the tenth in the revised scale of 1911:

(1) One day a man fell on his head off a bicycle and was instantly killed. He was taken to the hospital and they say he will never get better.

(2) I have three brothers—Paul, Ernest, and myself.

(3) Once the body of a poor girl was found in a wood, cut into eighteen pieces. They say that she killed herself.

(4) Yesterday there was a railway accident; but the newspaper says it is not a serious one as only forty-eight people were killed.

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[1] *J. of Exp. Pedag.* 1919, v. 73–77.

(5) A man once said, "If I should ever grow desperate and kill myself I shall not choose a Friday to do it on, for Friday is an unlucky day and would bring me bad luck."

In order to pass the test the child must detect three out of the five absurdities.

By way of criticism it may be remarked that four out of the five deal with murder or sudden death; that three depend on the same basal fact (the fact that a dead man is dead and can neither act nor suffer); that they are not equally easy; that they are not arranged in precise order of difficulty; and that there is some doubt as to the year to which they should be allocated. Burt considers No. 3 harder than No. 4, and No. 5 so hard that it is more suitable for children of eleven or twelve. Terman keeps the test in the tenth year, but Goddard and Kuhlmann place it in the eleventh year. Terman, however, makes two substitutions. Omitting No. 2 because in some languages the expression is idiomatically correct, and No. 5 because it is too hard, he substitutes the following:

(1) A man said: "I know a road from my house to the city which is downhill all the way to the city and downhill all the way back home."

(2) An engineer said that the more cars he had in his train the faster he could go.

To compensate for the easier items Terman requires four to be correct instead of three.

Of the other absurdities which have from time to time been suggested as alternatives to Binet's the best are these:

(1) This morning I met a smart young man. He was walking down the street with his hands in his pockets, and twirling a brand-new walking-stick. (Whipple.)

(2) When there is a collision the last carriage of a train is usually damaged most. So the guard thinks it would be best if the last carriage was always taken off before the train starts. (Yerkes.)

(3) An Irishman called one day at the post office and asked if there was a letter waiting for him. "What is your name?" asked the postmaster. "Sure," said the man, "you will find my name on the envelope."

(4) A boy wrote in his composition: "Soap smells nice but tastes horrid. It tastes worst of all when you get it in your eye."

The value of this type of test is beyond question. Those who have extensively used it agree with Terman, who says: "The detection of absurdities is one of the most ingenious and serviceable tests of the entire scale. It is little influenced by schooling, and it comes nearer than any other to being a test of that species of mother-wit which we call common sense." And again: "It is an invaluable test for the higher grades of mental deficiency."

Valuable as these particular tests are for detecting the dull-witted and for marking a certain stage of mental development, they are of little use in differentiating ordinary adults. Terman observes that by twelve years of age critical ability is so far developed that the test is nearly always passed. This may be so if it is given in the form prescribed by Binet; but if we alter the mode of administering the test we get quite different results. Binet's absurdity tests are oral and individual; the child is told beforehand that each sentence contains something that is silly, and he is asked to discover it. But if, on the other hand, the child is left in doubt whether the statement is sensible or silly; if he is told that it may be either the one or the other, and is asked to say which and give his reasons, and especially if he is asked to write those reasons down, the difficulty of the task is greatly increased. Binet's plan gives two possibilities—to find the absurdity or not to find it. The other plan gives three—to judge the sentence sensible, or to judge it absurd without being able to explain the absurdity, or to judge it absurd and to explain the absurdity.

The difference is exemplified by a test which I have frequently used: Captain Cook made three voyages round the world. In one of these voyages he was killed by savages. Which voyage was it—the first, the second, or the third? Although not precisely conforming to the type described above this is clearly an absurdity test admitting of three possible answers; and if, when the third voyage is given a reason is demanded the number of possibilities is increased to four. It is difficult to conceive that a lad of thirteen years of age with his wits about him could fail to give the right answer and the right explanation. Yet as a matter of fact about 40 per cent. of them do fail; at least they do so when the test is written and not oral. Few give the wrong voyage (although about five per cent. do even this), but those who select the third voyage cannot always show that they decide on logical grounds.

I therefore set about devising a series of absurdity tests which would be free from the main objections urged against the Binet absurdities, which would vary in difficulty and in the type of incongruity, and which could suitably be applied to older pupils.

4. THE TESTS USED.

The tests I used in my investigation comprise 34 absurdities mixed with a few quasi-absurdities—statements which at first blush seem absurd but are not really so. I give below the 34 real absurdities arranged in increasing order of difficulty. They are derived from various sources.

Some I have remembered, some (not many) I have invented, and some have been suggested to me by friends.

Instructions.—On the top of the paper write the name of your school, your name, and your age in years and months.

Some of the following sayings are sensible and some are foolish, and you must say which of them are sensible and which are foolish. Do not write out the saying, but simply put its number on your paper and write after it the word "sensible" or "foolish." If you write the word "sensible" you need say nothing more; but if you write the word "foolish" you must give a reason for your opinion.

Section A.

(1) A soldier writing home to his mother said: "I am writing this letter with a sword in one hand and a pistol in the other."

(2) It is said that a certain town in Greece contains two relics of St Paul; one his skull when he was a boy, and the other his skull when he was a man.

(3) An old gentleman complained that he could no longer walk round the park as he used to: he could now only go half way round and back again.

(4) In the year 1915 many more women got married than men.

(5) A hunter who had used up all his ammunition was chased by a bear. A bright idea struck him: he would climb a tree. When he got to the top he remembered that the bear could also climb a tree; but he got out of his difficulty by pulling the tree up after him.

(6) Light comes from the sun; feathers are light; therefore feathers come from the sun.

Section B.

(7) A boy who wanted to go to a cinema but had no money, thought it would be a good plan to walk in backwards, for the man at the door would think he was going out, and would not ask for his ticket.

(8) A teacher said to his boys: "Tomorrow we hold a scripture examination, attendance at which is voluntary; so if any boy is absent he had better look out."

(9) A soldier in the march complained that every man in the regiment was out of step except himself.

(10) A sailor who was put to haul in a rope from the sea found it so long that he gave it up in disgust saying that somebody must have cut the end off.

(11) The moon is more useful than the sun, for it gives us light in the night when we really need it, while the sun gives us light in the day when we don't need it.

(12) This is a sad and bitter world: we never strew flowers on a man's grave until he is dead.

(13) A stammerer who came from Birmingham to live in London was asked why he stammered more in London. He replied: "B-b-bigger town."

(14) A man said to his shoemaker: "You blockhead; I told you to make one of the shoes larger than the other, and instead of that you have made one of them smaller than the other."

(15) A speaker at a war economy meeting said: "We must all economise. The man who generally buys four suits of clothes a year should be satisfied with three, the man who buys three should be satisfied with two, and so on."

Section C.

(16) There is a tree in America which is so tall that it takes two men and a boy to see the top.

(17) A householder saw an advertisement: "Buy one of Simkin's stoves and save half your coal." He bought two in order to save all of it.

(18) A forgetful man once tied a knot in his hankerchief to remind him of something; but he suddenly remembered that the last time he did this he could not recall what it was he had to remember. So this time he tied two knots to make sure. The first knot was to remind him that he had to remember something; the second to remind him of what that something was.

(19) I am not conceited, for I don't think I am half as clever as I really am.

(20) A country man came up to London during the war and saw on a hoarding the words: "Eat less bread: do it now." He immediately went into a tea-shop to do it now.

(21) "I don't like onions," said the boy, "and I'm glad I don't; for if I did I should be eating them all day, and I hate the beastly things."

(22) An Irishman was charged at the police-court with having stolen a pig. Five witnesses swore that they had seen him do it; but the prisoner said that he could prove his innocence by bringing 50 witnesses who would swear that they had not seen him steal the pig.

Section D.

(23) "I have always noticed," remarked the old man, "that if I do not die in March I am all right for the rest of the year."

(24) Some years ago it was proposed to shift Sunday from the end of the week to the middle so as to divide the week into two parts.

(25) John James, who had married his widow's sister, used to say that if a man had a bad sister it was his misfortune, but if he had a bad wife it was his fault.

(26) You are thin, and I am thin; but he is thinner than both of us put together.

(27) One should never use long words when short ones will do just as well, for when the words are long the meaning is obscured by the unusualness of the terminology, and intelligibility is consequently endangered.

(28) The three men laughed; then stopped suddenly as the eyes of each met those of the others across the table.

(29) Watching pigs wallowing in the miro, he muttered: "No wonder they are called pigs."

Section E.

(30) A man who bought a dog that had been advertised complained to the seller that the dog's legs were too short. The seller replied: "They are long enough to reach the ground aren't they? what more do you want?"

(31) A showman advertised for a giant and a dwarf. A man of ordinary height presented himself and offered to fill both parts. He claimed to be the smallest giant in the world and the biggest dwarf.

(32) While standing near a clock tower just before the clock struck twelve two boys tried to find out which of them could hold his breath the longer. Neither of them won; for one was able to hold his breath from the first stroke of the clock to the sixth, and the other from the sixth to the twelfth.

(33) Every rule, even this one, has an exception.

(34) The horse obeys his master because his eyes magnify, so that his master seems to the horse to be much larger than the horse himself.

The plan I adopted in setting the test was to give each child a cyclostyled copy of 38 statements comprising the 34 real absurdities interspersed with four spurious absurdities. The four spurious ones, arranged in increasing order of difficulty, were:

(1) A man may live many years on a crust—the crust of the earth.

(2) An old woman said that she was so old that in a few years time she would be twice as old as she was forty years ago.

(3) I bought my little boy a watch which was said to give the right time twice a day; it turned out to be a dummy watch with fixed hands.

(4) The commonest word in the English language that is spelt the same way backwards and forwards is "a."

Any others of the same kind would have served just as well, for whatever answer was given it was not scored. Only the real absurdities were considered in estimating the results.

The time allowed was unlimited. The subjects were asked to hand in their papers as soon as they were finished.

The sectional classification of the absurdities is based on the percentage of adults who succeeded in detecting them. Each question in Section A was correctly answered by over 80 per cent. of adults, each in Section B by between 60 and 80 per cent., each in Section C by between 40 and 60 per cent., each in Section D by between 20 and 40 per cent., and each in Section E by less than 20 per cent. Only 6 per cent. dealt successfully with the last question.

The test was given to all children over eleven in two elementary schools, two central schools, three secondary schools, one day continuation school, one evening school, and one training college. Through the kind help of Dr R. R. Rusk I was able to secure results from a few schools and a training college in Scotland. Altogether about 2000 subjects were tested[1].

5. METHOD OF MARKING.

The papers were all marked by myself, or under my supervision, in accordance with the following key[2]:

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[1] I wish here to record my obligations to the following principals and head-teachers for their valuable co-operation in this research: Miss W. Mercier, M.A., Miss M. A. Tucker, M.A., Rev. C. J. Smith, M.A., Mr E. B. Cumberland, B.A., B.Sc, Mr J. G. Robson, Mr T. Lea, Mr T. G. Tibbey, M.A., Mr F. E. Rogers, B.A., Mr E. G. Dixon, and the Principal of Harrods' Continuation School.

[2] My thanks are due to my colleague Mr F. P. B. Shipham, M.A., for his help in the marking and revision of the papers.

KEY.

1. ***Right:*** "He had no hand free for writing." "Both hands occupied," etc.
   Wrong: "He couldn't write with a sword."
2. ***Right:*** "No man has two skulls." "A man has the same skull as he had as a boy," etc.
3. ***Right:*** "The distance is the same."
   Wrong: "He was a silly old man."
4. Some may contend that there is nothing inherently absurd in this, since a widow, widower, bigamist, or a divorced person may disturb the balance. Such an answer is to be considered correct.
5. Two points: (i) "The impossibility of pulling up the tree if he was on it," (ii) "The futility of doing it." If one point is given the test is passed.
   Right: "He could not pull up the tree if he was on it."
   "He had nowhere to put his foot except the tree."
   "He had no fulcrum."
   "If he pulled the tree up he would fall down with it."
   Wrong: "He could not pull the tree up."
6. The logical fallacy must be detected.
   Right: "The word light is used in two senses."
   "When we say feathers are light we mean weight."
   Wrong: "Feathers do not come from the Sun."
7. The answer must imply that the boy would still be moving inwards.
   Right: "The man at the door would see that the boy was going in not out."
   Wrong: "The man at the door would stop him."
8. ***Right:*** "Voluntary means, if you like."
   Wrong: "The teacher should not punish a boy for being absent."
9. ***Right:*** "He was out of step himself."
10. ***Right:*** "By cutting the end off the rope is made shorter."
    "The rope still has an end."
    Wrong: "You can't cut the end off."
11. ***Right:*** "It is the sun that makes the day."
    "Without the sun there would be no day."
    Wrong: "The moon gets its light from the sun."
12. ***Right:*** "A man has no grave until he is dead."
13. ***Right:*** "The size of the town makes no difference."
14. ***Right:*** "You cannot have one larger without having the other shorter."
    "Both statements mean the same."
15. ***Right:*** "The man who had one suit a year would have to be satisfied with none."
16. ***Right:*** "Two men and a boy cannot see further than one man or one boy."
    "We cannot add sights together."
    Wrong: "Anybody could see the top."
17. ***Right:*** "Two stoves would burn more than one."
    "To save all his coal he must burn none at all."
18. ***Right:*** "Two knots would not make him remember better than one."
    "If the first knot would not remind him the second would not either."
    Wrong: "Knots can't make you remember."

19. *Right:* "He contradicts himself."

"He first says that he is not conceited and then says that he is."

"He says he does not think himself clever and then says that he does."

Any answer will do that points out a contradiction, (*a*) between the two clauses or (*b*) between the two parts of the second clause.

20. *Right:* "He could not eat less by going into a tea-shop: he could only eat more."

"The advertisement asked you to eat less at meal times: the 'now' did not mean at this moment."

21. *Right:* "The boy assumes that he likes onions and that he does not like onions at the same time."

"He contradicts himself. He says, If I like them I should hate them."

22. *Right:* "The 50 witnesses were not real witnesses: they did not see anything."

"Only those who were present could give real evidence."

Any answer will do if it points out that negative evidence is in this case of no value.

23. *Right:* "He might have said the same of any other month in the year."

The fallacy consists in asserting that to be true of a particular month which is equally true of any other month.

24. *Right:* "There would still be a week from Sunday to Sunday."

"It would not split the week."

Wrong: "You can't shift Sunday."

"Sunday is the beginning of the week, not the end."

25. *Right:* "If a man has a widow he is dead."

26. *Right:* "Putting men together makes them fatter not thinner."

27. *Right:* "The writer does not follow his own advice."

"He tells us to use short words, and he himself uses long ones."

28. *Right:* "One man's eyes cannot meet two other men's eyes at once."

29. *Right:* "Pigs are not so called because they are dirty, but it is the other way about."

"Pigs is their proper name, their dirtiness has nothing to do with it."

Any answer will do if it suggests that the name is prior.

30. *Right:* "Every dog's legs reach the ground" (implying that the buyer's objection to this particular dog is not met).

"Legs half an inch long would reach the ground, but they would be of no use."

"The buyer meant that the legs were not long enough to be useful or beautiful."

"What the buyer meant was that the legs were not long enough to keep the body some distance from the ground."

31. *Right:* "A giant must be above the ordinary height, and a dwarf below the ordinary height."

"Smallness is a bad quality in a giant, and largeness in a dwarf."

32. *Right:* "The second boy won for he held his breath for six intervals, while the first held it for five."

33. *Right:* "Self-contradictory."

"This itself is a rule and the exception to it is that there is a rule which has no exception. Therefore if the rule is true it is also false."

34. *Right:* "If his eyes magnify they magnify everything in the same proportion."
"If he sees a man big he also sees another horse big, and sees his own body big."

6. RESULTS.

The first school tested was a Central School, a school to which selected children are annually drafted from a group of contributory schools. The selection is based on proficiency in arithmetic and composition as tested by a general competitive examination. There are no grounds for thinking that one year's draft is any better or any worse than another year's draft. The results are given in Table I.

Table I.

Scores gained at a Central School.

Ages	11–12	12–13	13–14	14–15
No. of children ...	80	108	124	40
Average score ...	14·9	17·4	16·6	16·8
Median score ...	15·5	17·5	16·7	17·0
Semi-interquartile range	3·1	2·2	2·7	2·5

The number of children over 15 was so small that no importance is to be attached to the results obtained from them. It is to be noted that the twelve-year-old group did better than any other.

At another Central School, however, to which the test was applied the scores improved with increasing age, for the average marks (with the semi-interquartile range (S.I.R.) in brackets) obtained by the age-groups beginning at 11–12 were 12·9 (1·9), 14·3 (3·0), 14·4 (2·5) and 16·5 (2·4), respectively.

The three age-groups beginning with 11–12 were tested at an ordinary elementary school, and the average scores made were 8·5 (S.I.R. = 3·2), 10·7 (S.I.R. = 2·2), 10·2 (S.I.R. = 3·0). The twelve-year-old's did better than the thirteen-year-old's. At another elementary school, however,—a school in a better neighbourhood—the corresponding scores were 10 (S.I.R. = 4·2), 12·6 (S.I.R. = 2·2) and 13·6 (S.I.R. = 2·7).

In a large and good secondary school the averages for the years 11 to 17 respectively were: 18, 18·8, 19·7, 22·6, 22·8, 22·5, 22·3. The semi-interquartile range of all the scores was 2·8. The maximum score was reached at 15. At another secondary school the maximum was again reached at 15, though there were pupils up to 18.

Interesting results were secured at a continuation school held on the premises of a large business firm. There were 34 pupils in the fourteen-year-old group, 164 in the fifteen-year-old group, and 112 in the sixteen-year-old group. The average marks were 19·1 (S.I.R. = 3·3), 15·6 (S.I.R. = 2·7) and 16·7 (S.I.R. = 3·3) for the three groups respectively. The superiority of the youngest group is explained by the fact that they were the only group selected by the firm after peace had been signed. During the war the selection was necessarily less stringent.

At a training college for women 141 students were tested. The average score was 21·1, the median was 22, and the semi-interquartile range was 3·5. The results indicate a degree of intelligence well above the average, but a little below the average at a good secondary school; thus confirming an opinion, frequently expressed, that the best scholars from secondary schools do not enter the teaching profession.

The number of papers received from Scotland was too small for the results to be significant. Such as they were, however, they indicate that Scottish children have keener eyes for absurdities than English children. The popular belief that a Scotsman cannot see the point of a joke is probably ill-founded: he sees the point but he does not see its fun. It is an emotional, not an intellectual disability.

The validity of pooling results from such disparate sources is very doubtful; and too much weight must not be attached to the following scores which were obtained by taking all the papers together from all the schools tested in England and Scotland, and finding the average:

Age	11	12	13	14	15	16	17
Averages	13·1	14·4	15·1	17·4	18·5	18·9	18·9

These norms support the conclusions reached by other investigators. Remembering what has already been said about the tendency of the duller children to fall out of the ranks after fourteen, we feel justified in concluding that the rate of growth of intelligence gradually slows down and that after sixteen years of age it virtually stops. Indeed there is not much improvement after twelve. It is true that my figures show a fairly large step from 13 to 14, but this is easily accounted for by the fact that the thirteen-year-old's tested were a fairly representative group; while after the fourteenth birthday social and intellectual selection began to operate.

An attempt was made to compare the abilities of secondary school boys with those of older people. A test for this purpose was prepared by selecting ten of the absurdities given above (the first and last in each of the five sections), and one quasi-absurdity—the third. The results of this test, which was given at a secondary school for boys and an evening school for men and women, and set forth in Table II.

The marks gained by the evening school students were amazingly low. These students, however, were not of an intellectual type; and there is a possibility that some of them did not take the tests quite seriously. The mistakes they made did not as a rule consist in the wrong interpretation of an absurdity, but in a sheer inability to see that there was an absurdity there at all. The item was simply labelled 'sensible.' There was no attempt at irony, sarcasm, or 'smartness' of any kind, such as was occasionally made by the secondary school boys.

Table II.

Marks obtained out of ten.

Secondary School.

Age	16–17	17–18	18–19
No. of pupils	45	15	13
Average score	7	5·8	6·9
Semi-interquartile range	1·4	1·7	·9

Evening School.

Age	15–17	17–19	19–22	over 22
No. of students	17	39	18	19
Average score	3·8	4·2	3·8	3·1
Semi-interquartile range	·8	1·2	1·0	1·5

The evidence on which to base a judgment of adult intelligence is regrettably scanty. The number of adults tested by me is small, and there is no guarantee that the sample is good—good as a sample. Yet scanty as the evidence is, it strongly forces on us the conviction that intelligence ceases to grow about the age of sixteen; for even when there are *a priori* grounds for believing the samples beyond sixteen to be weighted on the intellectual side we often find no sign of improvement in intelligence.

7. THE CURVE OF GROWTH OF INTELLIGENCE.

The validity of the above results rests on the assumption that the successive age-groups tested are on the average of the same intelligence quotient—that the fifteen-year-old subjects, say, will be on the whole exactly as intelligent a year hence as the sixteen-year-old subjects are to-day. Only thus can we regard the curve determined by the aggregate results as indicating the normal growth of intelligence. That these conditions were in my investigation fulfilled continuously from the ages of eleven to eighteen cannot be asserted. All that can be asserted is that in the individual schools tested there were no grounds for thinking that the quality of the minds varied from year to year up to, and including, the thirteen-year-old group; and as for the higher age-groups there were no grounds for thinking that the quality deteriorated, though, as I have already pointed out, there were good grounds for thinking that it improved.

Assuming that the tests I have used really measure intelligence, and combining my conclusions with those reached by other investigators, I may state generally that the growth of intelligence, which is rapid for the first few years, gradually slows down as the child gets older, that after twelve years of age the slowing-down is very marked, and that after sixteen further growth is inappreciable. It is this retardation of growth that accounts for the fact that Binet failed to interpolate in his scale a set of tests either between the ages of ten and twelve or between the ages of twelve and fifteen: omitting the doubtful tests for adults he could only mark out two steps after the age of ten.

It follows that a year of mental age is not a fixed unit: it gradually diminishes towards the higher end of the scale.

If after sixteen years of age the curve of growth is virtually a straight line, as it seems to be, we are forced to conclude that sixteen is not the average age at which growth ceases, but the maximum age. For the general curve is the resultant of a large number of individual curves; and if the resultant curve is straight after sixteen, then either all the individual curves are straight after sixteen, or if some rise others must fall. In other words we must choose between two alternative suppositions, one that nobody improves after sixteen, the other that some people in sound health begin at that age to suffer from senile decay. There is evidence for thinking that different races and different individuals vary in the age at which mental maturity is reached. S. D. Porteus[1] found that Australian aborigines complete their growth in intelligence several years earlier than white children. The average mental age of recruits in the American army during the war is said to be thirteen. It is highly probable therefore that the average age at which growth ceases is considerably below sixteen. The exact determination of this average age is of importance in estimating the intelligent quotient of adults; for if, following Terman, we use sixteen as the denominator for all chronological ages over sixteen, we undoubtedly get a quotient which is too low.

There is obviously need for an investigation which aims at finding the separate curves of growth for a large number of individuals, an investigation that will be wide in its scope and will be carried over a long series of years. It is only by an extensive research of this kind that we can find an answer to the question: What connexion is there between the quality of a person's mind and the age at which his intelligence reaches maturity? Are 'late bloomers' better than 'early bloomers'?

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[1] "Mental Tests with Delinquents and Australian Aboriginal Children." *Psychol. Rev.* xxiv. 32.

There is also need for testing the intelligence of representative samples of adults at, say, 20, 30 and 40 years of age. It is just possible that the growth of intelligence after sixteen, imperceptible as it is from year to year, may in ten years reach an amount that may be easily measured.

8. THE FURTHER USE OF THE TESTS.

Those who wish to repeat my experiments with the same tests can use either the whole series, or any selection that would present the same difficulty as the whole series. As the questions are arranged in order of difficulty this selection may be made by taking alternate items, or every third item, or the middle item of each section, or two items from each section equally removed from the middle of that section, or any other arrangement that contains a balanced number of easy and difficult items. Having made a selection, however, it would be well to adhere to it for the whole inquiry; for the series is not so carefully standardised that the items rise in equal steps: selections made in accordance with the above instructions will be approximately, not exactly, of equal difficulty. The objection to using a small number of items is that a few bright subjects may get them all right. This is not likely to occur if the full series be used. None of my own subjects got more than 32 marks out of the total of 34. The absurdities should be mixed with a few pseudo-absurdities, either those given above or any others of a similar kind: but the pseudo-absurdities should be ignored in marking the papers.

The percentage of absurdities detected and explained gives the grade of intelligence. The normal adult seems to get about 50 per cent. of the marks.

9. THE NATURE OF INTELLIGENCE.

The Editor of *The Journal of Educational Psychology* recently invited a number of leading investigators in America to state what they meant by 'Intelligence,' and the replies sent in by thirteen of them appear in the issues of the *Journal* for March and April, 1921. These replies reveal a surprising variety of opinions. No common nucleus of meaning is discoverable, unless it be that Intelligence is something that is measured more or less exactly by Binet's scale and by the American Army tests. Nor do the views of European psychologists show much more unanimity. It is possible, however, to group the various opinions under three heads: those that regard intelligence as a single ability common to all intellectual processes; those that regard it as a group of two or three abilities of varying degrees of generality; and those that regard it as representing no real entity but as merely a convenient term for the average of all specific abilities.

Of those writers who hold the first theory many accept Stern's[1] definition: "General adaptability to the new problems and conditions of life." Burt[2] defines intelligence as "inborn, all-round mental efficiency." Woodrow[3] calls it "a capacity to acquire capacity"—a phrase that suggests an endless regression. Spearman[4], whose name is most closely connected with this theory prefers calling the central factor "general ability" (symbolised by g) without committing himself to the view that g is identical with intelligence as commonly understood.

The second theory, the group theory, is held by Binet[5] who regards intelligence as involving at least three factors which are thus summarised by Terman[6]: "(1) Its tendency to take and maintain a definite direction; (2) the capacity to make adaptations for the purpose of attaining a desired end; and (3) the power of auto-criticism." Another interesting view of the composite nature of intelligence is suggested by Maxwell Garnett[7] who says: "It is probable that genius, as the word is commonly understood, is more directly measured by $\epsilon = \sqrt{g^2 + c^2}$ than by g (measuring General Ability or capacity to concentrate attention) alone, or even by c (measuring Cleverness, as we have defined it, or tendency to associate by similarity) alone." In other words the general factor g is compounded with one, or perhaps more than one, group factor to constitute Intelligence.

The third theory, the theory of the independence of the several traits that go to make up the mind, has been advocated with wavering confidence by Thorndike[8]. Though, in his latest pronouncement, he admits a certain possibility of prophesying from a test score "how well a person will do in other intellectual tasks," he warns us that "to assume that we have measured some general power which resides in him and determines his ability in every variety of intellectual task in its entirety is to fly directly in the face of all that is known about the organisation of intellect." Under the same head comes G. H. Thomson's[9] Sampling Theory of Ability which assumes that each man's mind is a sample group of qualities, resembling Mendelian units, which come from the hereditary stock. "If there be a General Factor at all, it might be the power to shake down rapidly into good team work."

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[1] *J. of Educational Psychol.* XII. 127.

[2] *J. of Experimental Ped.* I. 95.

[3] *J. of Educ. Psychol.* XII. 208.

[4] This *Journal*, V. 51–84.

[5] "L'intelligence des imbeciles," in *L'Année Psychologique* (1909), 1–147.

[6] *Opus cit.* p. 45.

[7] *Education and World Citizenship*, 125.

[8] *J. of Educ. Psychol.* XII.

[9] *Essentials of Mental Measurement*, by Brown and Thomson, 1921, 188–192.

In spite of this divergence and apparent inconsistency of opinion, many of the individual views seem merely to accentuate one or more aspects of a complex whole. At any rate the majority of the theorists would probably subscribe to the following tenets: Intelligence is innate mental ability which operates in many different ways; it is more fully manifested in the higher mental processes than in the lower; it is specially active in dealing with a situation which presents points of novelty (in other words with the solution of problems); it is more concerned with the dissecting, planning and re-arranging of the data of experience than with the mere reception of impressions.

The aspect or mode of intelligence which absurdity tests call into play seems to be the power to construct coherent wholes. It is that synthetic power which Bosanquet[1] regards as essential for inference. "Ultimately," he says, "the condition of inference is always a system.... a group of relations of properties or things so held together by a common nature that you can judge from some of them what the others must be." Some indeed require the system to be apprehended as a spatial and concrete thing before they are satisfied. Lord Kelvin[2] did not at first accept Clerk Maxwell's electromagnetic theory of light, because he could not make a model of it. An absurdity test contains an incongruity—something that does not fit; and the failure to fit becomes apparent only when all the pertinent elements are brought together in the mind to form a whole. In these tests all the elements of knowledge requisite for a solution are just as familiar to the youngest testee as they are to the oldest. The oldest is robbed of his advantage of a larger store of knowledge, so that any difference in the response is due to a difference in synthetic power—provided the mental attitude is the same. If the attitude of the testee is one of suspicion; if he looks upon the test as a 'catch' and not a *bona fide* question; if he is so wary that he avoids the answer that appears most obviously right, it is clear that the results cannot be taken at their face value. When this frame of mind is suspected steps should be taken to dispel it before the test is given.

The probability being that intelligence is a composite thing, and means more than synthetic power, then the most that can be claimed, as the outcome of my investigation, is that, given a normal environment, a certain factor of intelligence—the power to integrate experience—arrives at maturity when the subject is sixteen years of age or younger. How far this factor correlates with the other factors—if there are other factors—this investigation does not show. From the way, however, in which Binet's absurdity tests fit in with the whole scale and correspond with the teacher's estimate[1], there is every reason to think that the correlation is high, and that no single type of test can gauge intelligence better than the absurdity test.

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[1] *The Essentials of Logic*, 140.

[2] *Life of Lord Kelvin*, by Silvanus P. Thomson, II. 835–36.

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[1] See Burt's forthcoming book, ***Mental and Scholastic Tests***, Tables III and XXXI.

(*Manuscript received* 29 *March*, 1921.)

A VINDICATION OF THE RESONANCE HYPOTHESIS OF AUDITION.

II.

By H. HARTRIDGE.

(From the Cambridge Physiological Laboratory.)

1. *When the periodic stimulus applied to a vibrating resonator is reversed, or changed by π, the resonator must first come to rest and must then restart in time with the new period* (pp. 142–145).
2. *When a musical note is altered in phase by π the observer hears the sound die away momentarily to return a moment later to its original intensity* (p. 145).
3. *These facts cannot be explained unless there is a resonating mechanism somewhere in the auditory apparatus; and therefore Wrightson's theory cannot explain them* (pp. 145, 146).

1.

In a previous paper[1] I have described experiments explicable by the resonance theory (Helmholtz) but at variance with the displacement theory (Wrightson's) of hearing. I have also in the same paper answered Perrett's objections[2] to the resonance theory.

In this paper further experimental evidence will be given which is in favour of the resonance hypothesis and at variance with its antagonist. This evidence is based on the principle that if the periodically applied forces, which are causing a mechanical system possessing resonance to vibrate, are reversed, or altered in phase by π, then the motion of that system must first be arrested and must then be restarted in the new phase. As examples of this principle the case of a body of men crossing a suspension bridge and setting it swinging may be cited; if the men change step the motion of the bridge will first be arrested and will then be restarted again in time with the new step. This process will be repeated each time the step is changed.

It should be noted that any mechanical system that is to exhibit this phenomenon must possess a natural period of vibration and the stimulus undergoing the change of phase of π must have the same rate.

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[1] This *Journal*, 1921, LV. 277.

[2] *Questions of Phonetic Theory*, 1919, ch. V, 32.

With regard to the different mechanical structures which together compose the ear, all authorities are now agreed that the drum, its ossicles and muscles together, form a mechanism which is dead beat and has no apparent natural period. With regard to the organ of Corti of the cochlea one authority (Helmholtz) states that certain parts (the different fibres of the basilar membrane) have natural periods of their own, whereas other authorities (e.g. Rutherford and Wrightson) postulate that this mechanism is dead beat.

So far as I am aware neither authority makes any statement as to the kind of mechanism (if any) in the auditory area of the brain where the nervous impulses are interpreted as sensations.

The essential difference then between the rival theories is that one (Helmholtz's) says that the ear contains a mechanism, parts of which have natural periods and act as resonators, whereas the other denies this. Now it would seem that the principles outlined above could be applied to the problem of providing a crucial test on this point. For if on suddenly changing the phase of a certain musical note by π there is observed to follow a temporary cessation of the note followed by its return to the normal, then this would seem to give definite evidence for the presence somewhere in the ear of a resonator mechanism, the natural period of which corresponds to that of the note. If further all musical notes behave in a similar manner, then there must be a series of such resonators in the ear. If, on the contrary, a change in phase of π made in turn in a series of notes, is found to produce no audible effect, then it would seem clear either that no resonators having natural periods are to be found in the ear, or alternatively, that the sensations conveyed by the auditory nerve lag so far behind the change in amplitude of motion of the hair cells that the temporary stoppage of the motion is not consciously recognised. Now we know from other experiments that even slight changes in amplitude are very readily perceived, the alternative mentioned above is therefore clearly ruled out.

The physical difficulties of changing the phase of a musical note suddenly by π, without at the same time introducing some accompanying change, were found to be very great. Several experimental methods had to be abandoned in turn either because the change could not be effected without noise or because the intensity or quality (or both) of the note were altered. The only method found satisfactory was one employing the de la Tour's syren for producing the musical note.

The change in phase of π was effected as follows: The mounting of the wind chest of the syren was modified so that a rotation could be effected about a vertical axis (the syren disc shaft was also vertical). There were eighteen holes in both the syren disc and the wind chest, so that this number of puffs of air were produced during each complete revolution of the disc. The angular interval between successive puffs was therefore 20 degrees ($360° \div 18$). By means of fixed stops (covered with rubber to absorb shock and so prevent noise) the rotation of the wind chest was limited to one half the above angle, viz. 10 degrees.

Supposing then the syren disc to be rotating to the right at a speed of 100 per second, then there will be 1800 puffs of air produced per second and a uniform note of 1800 vibrations per second will be the result.

Suppose now, that a puff of air has at a given instant issued from the holes in the disc and that immediately afterwards the syren disc is suddenly rotated 10° in the *same* direction as the disc, then the disc will have to rotate $20° + 10°$ before the next puff can be produced. That is to say, the normal time interval between the puffs is increased from T to $\frac{T \times 3}{2}$, or from $\frac{1}{1800}$ second to $\frac{1}{1800} \times \frac{3}{2}$.

Now in the normal note compression of the air follows compression at an interval of $\frac{1}{1800}$ and therefore rarefaction follows compression at an interval of $\frac{1}{1800} \times \frac{1}{2}$. If then by rotating the wind chest an interval of $\frac{1}{1800} \times \frac{3}{2}$ (i.e. $\frac{1}{1800} + \frac{1}{1800} \times \frac{1}{2}$) is introduced, it is clear that the compressions in the air in the note which subsequently follows must occur at those instants which would have been occupied by rarefactions and *vice versâ*; that is that a change of phase of π has been introduced.

In the same way when the wind chest is rotated 10° in the *opposite* direction to the disc (so that the holes tend to approach one another more rapidly), the disc only need rotate 10° for a fresh set of holes to coincide and therefore the time between the puffs is reduced from T to $\frac{T}{2}$, i.e. in the case we have assumed from $\frac{1}{1800}$ sec. to $\frac{1}{1800} \times \frac{1}{2}$.

It follows therefore that in this case also compression of the air will occur at the instant that rarefaction would have occurred and therefore a change in phase of π will have been introduced.

We have now to consider what will happen if the rotation of the wind chest cannot be effected suddenly, e.g. suppose the rotation of 10° to take one second. Then it will be found that during this second $1800 + \frac{1}{2}$ (or $1800 - \frac{1}{2}$) puffs will have occurred, that is that the frequency of the note is 1800·5 (or 1799·5) per second, according as the wind chest is rotated in the opposite or the same direction as the disc. The note during rotation of the wind chest will therefore be found to become slightly sharp during the instant that the wind chest is rotated one way and slightly flat during the instant that it is rotated the other.

Observation shows that as the time required to rotate the wind chest is shortened at first, the more obvious does the change in pitch become; but as the time is further decreased the less obvious does it become, until finally when the change is made as suddenly as possible the momentary alteration in pitch passes unnoticed.

2.

A new phenomenon however is now observed; for when the change in phase of the note is effected, the intensity now suffers a sudden diminution, while the pitch remains unaltered. After having fallen to zero intensity, the note rapidly recovers its original strength or even momentarily reaches a higher loudness than before.

Physiologically the phenomenon has the characteristics of a beat (that produced for example when two organ pipes sounding together have a slightly different pitch) and is best so described. It is found to occur equally well with all notes within the range of the syren, and it is readily heard by a large audience.

The following observations show that the beat is not of instrumental origin: (1) If the wind chest be rotated slowly when the syren is in operation no beat is heard. (2) If with the syren in rotation the wind be momentarily turned off and the wind chest be then rotated either slowly or fast no beat is heard.

3.

The presence of this beat is readily explained on the resonance hypotheses because the change in phase of a musical note by π will put it out of step with the resonators which it has set in motion. These resonators must first be brought to rest and must then commence vibrating again until once more they reach a steady state. The temporary rise of the intensity of the note above the general level after the beat, may be due either to error of judgment, successive contrast, or possibly to temporary excessive vibration on the part of the resonator soon after the recommencement of its motion.

It would appear very difficult to explain the presence of the beat on any displacement hypotheses. If it is suggested that the beat is really a short silent interval brought about by an increase in time between the successive impulses from T to $T \times \frac{3}{2}$ which the ear perceives owing to its dead-beat properties, then it can be objected that in the first place experiment shows that a short interval of silence, produced artificially in a musical note, is perceived by the ear as a short noise[1] (a phenomenon itself already explained on a resonance but not on a displacement hypothesis), and that in the second place it is only when the wind chest of the syren is rotated one way, that the time between successive impulses is temporarily increased, when on the contrary it is rotated the other way, the impulses should come closer together and therefore on the displacement hypothesis no silence should be produced; yet experiment shows that the beat is as clearly heard in this case as it is in the other.

Two criticisms can therefore be advanced against the displacement hypothesis: (1) that it does not explain why a short silence in a musical note is heard as a short noise apparently added to the uninterrupted note, and (2) that it does not explain the beat which is produced when the phase of a musical note is suddenly altered by π.

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[1] This *Journal*, 1921, XI 277.

(*Manuscript received* 4 *July*, 1921.)

A PORTABLE CHOICE REACTION TIME APPARATUS.

By A. G. SILLITOE.

(*From the Cambridge Psychological Laboratory.*)

The apparatus about to be described is a development of the instrument devised by the late Dr Stamm for testing the reaction-times of aviators during a flight. The special features of this development are: the lightness and portability of the apparatus, and the new electrical control of the stop-watch.

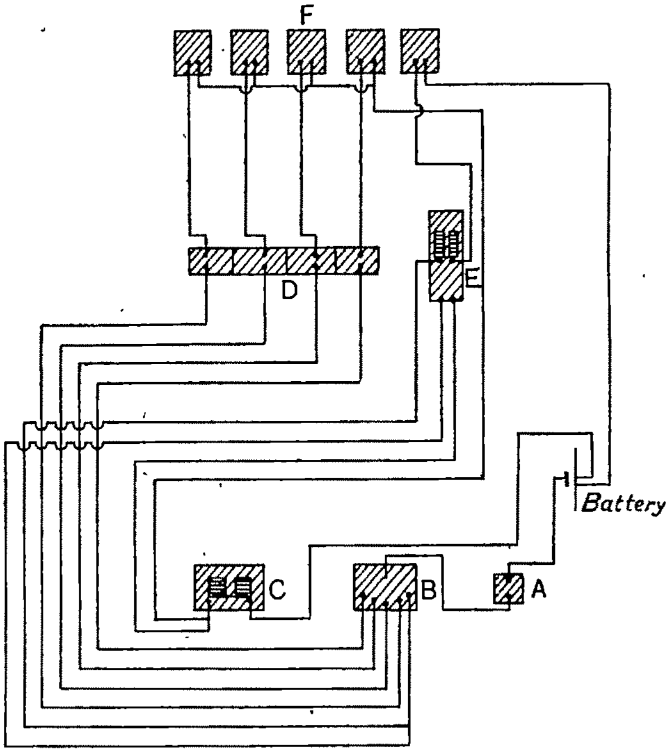
The whole is enclosed in a box 12″ × 8″ × 8″, its total weight being about 8½ lbs. Two opposite sides of the box are hinged, and may be opened outwards, so that when the apparatus is in use these sides lie flat on a table. To them most of the working parts of the apparatus are affixed: they will be referred to as the experimenter's side, and the subject's side respectively.

On the experimenter's side is a 100th secs. "Mikograph" stop-watch (*C* in diagram), firmly fixed in position, and controlled (as will be explained later) by an electro-magnet placed immediately below the watch. On the right of this is a five-way switch *B* connected with the various stimuli, and on the right of the five-way switch is the experimenter's switch *A* for exhibiting a given stimulus and starting the watch.

On the subject's side are situated, in a position convenient for the fingers, five keys *F* after the Morse key pattern. Any of these, when depressed, stops the watch and, if the correct reaction is made, interrupts the current which exhibits the stimulus, each key being connected with its own stimulus. Underneath the topside of the box and facing the subject are four small compartments *D*, each containing a 2 v. lamp, the front of the compartments being fitted with red, blue, green and white glass respectively. Fitted inside the box, on the subject's left, is a small specially constructed hammer *E* for use as a sound stimulus. The experimenter has then a choice of five reactions to one of five possible stimuli.

10—2

The box contains two dry cells and two spare cells. The mechanism is satisfactorily operated by a current of approximately 2·5 volts.



A. Experimenter's Switch. *B.* Five-way Switch. *C.* Electro-magnet operating watch. *D.* Visual Stimuli. *E.* Sound Stimulus. *F.* Subjects' Reaction Keys.

DIAGRAM OF CONNEXIONS.

The mechanism for starting and stopping the watch may be briefly described. Connected with the armature of an electro magnet is a light lever which is fixed outside the watch, but passes through an opening drilled in the watch case. In the resting positions, when no current is passing through the magnet, this lever allows a spring which is a part of the ordinary mechanism of the watch, to exert pressure upon a revolving serrated wheel, thus bringing the wheel out of gear with a second serrated wheel. The second wheel is therefore now at rest, and as the hand which revolves around the dial-face is fixed on the pinion of this wheel, no time record is taken. At the beginning of the experiment, then, the watch is started, as in an ordinary stop-watch, by pressure upon the lever at the top; but the hand does not revolve until the current is allowed to pass through the electro-magnet. When this happens, the attraction of the armature prevents the aforesaid spring from exerting pressure upon the revolving serrated wheel. The latter is therefore, by the operation of the ordinary starting mechanism of the watch thrown into gear with the wheel which carries the dial hand, and the time record at once begins to be taken.

In a long series of experiments either the watch may be kept going the whole time, the time of each reaction being read off as in the case of the Hipp Chronoscope, or, if it is preferred, the hand may be restored to zero after each reaction by pressure upon the lever at the top of the watch.

The apparatus has been given a large number of trials in practical work in the laboratory and has been found highly reliable and satisfactory. In a long series of trials with the control hammer, the mean variation for an average time of 526^σ was found to be 10^σ.

(*Manuscript received* 1 *August*, 1921.)

FEELING-TONE IN INDUSTRY

(*A Report to the Industrial Fatigue Research Board.*)

BY B. MUSCIO.

I. *The 'Feelings of Fatigue'* (pp. 150–152).
II. *Experimental Data* (pp. 152–158).
III. *Discussion* (pp. 158–161).
IV. *Summary* (pp. 161, 162).

I. THE 'FEELINGS OF FATIGUE'

AT an early stage in the investigation of 'fatigue' it became clear that the 'feelings of fatigue' were not reliable criteria of the 'capacity to work.' Rivers states this conclusion in the following words: "In the performance of mental work especially, decided sensations of fatigue may be experienced when the objective record shows that increasing and not decreasing amounts of work are being done; and there may be complete absence of any sensations of fatigue when the objective record shows that the work is falling off in quantity, or in quality, or in both[1]." And Thorndike writes: "Kraepelin has emphasised this fact [that the feelings of fatigue are not measures of mental ability] but chiefly in connexion with the claim that we can be mentally fatigued without feeling so. What I wish to emphasise is that we can *feel* mentally fatigued *without being so*[2]."

In dealing with the data the general character of which has been expressed in the above quotations, the factors actually correlated have been *output* and 'feelings of fatigue.' The output, however, has always been *experimentally obtained*, and under conditions of *maximal effort* on the part of the subject; and in such experimental conditions it has usually been regarded as the best attainable measure of 'fatigue.' But 'fatigue' investigations have been chiefly concerned with the attempt to find a *test* of 'fatigue.' Consequently—since (1) output was accepted as a measure of 'fatigue' and (2) output and 'feelings of fatigue' could not be correlated by any straightforward formula—'feelings of fatigue' were altogether abandoned as a possible 'fatigue' *test*. And, largely because of the preoccupation of 'fatigue' investigations with the question of a 'fatigue' *test*, the 'feelings of fatigue' have come to be almost entirely disregarded, not only in 'fatigue' *test* investigations, but also in the investigation of the characteristics of normal working activity; and their exclusion from these investigations has appeared the more final in view of the failure of physiological psychology up to the present to arrive at a plausible conception of the physiological concomitants of 'feeling.'

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[1] *The Influence of Alcohol and other Drugs on Fatigue*, 2.

[2] *Psychol. Rev.*, 1900, VII, 481.

So far as the question of a 'fatigue' *test* is concerned, this position doubtless is final, because of general considerations affecting the question as well as the special relation of the 'feelings of fatigue' to output. But from the point of view of understanding what happens in much of the normal work of the world, these feelings seem far from negligible. They might indeed be regarded as negligible if their correlation with output were *nil*, and many statements could be quoted which appear to be based upon this assumption. But though what might be called the paradoxes of activity feelings are striking, it would be false to say that 'feelings of fatigue' bear *no* relation to activity or to output. If the 'feelings of fatigue' were to be used as a *test* of 'fatigue,' the coefficient of correlation between the two factors ('fatigue' and 'feelings of fatigue') would need to be approximately + 1; but to say that these feelings cannot be used as a 'fatigue' *test* is not to imply that the correlation between the two factors is approximately ·00. And, in fact, if 'fatigue' be measured indirectly by reference to output, this correlation would certainly be considerable[1], as common experience shows, and as is obvious from the data given below. This being so, there are possibly many characteristics of normal human 'work' that find their explanation in human *feelings*. For instance, any fall in normal industrial output curves may be very largely due, in a sense to be explained below, to the painful feeling-tone of 'fatigue' feelings. The data now to be presented bear upon this point.

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[1] Cf. T. Arai, ***Mental Fatigue***, 114: "we conclude that the feeling of fatigue is somewhat, though far from perfectly, correlated with the state of mental inefficiency." It was found, for instance, that in memorising German equivalents of English words the average time taken per word when the subject (the writer herself) felt 'good' was 15·7″ (A.D.=4·1″), when she felt 'medium' it was 16·9″ (A.D. = 3·5″), and when she felt 'tired' 18·8″ (A.D. = 5·37″). In mental multiplication of a 3-place by a 3-place number the time taken per example when the subject felt 'good' was 138·8″ (A.D. = 30·6″), when she felt 'medium' 142·9″ (A.D. = 20·6″), and when she felt 'tired' 151·3″ (A.D. = 28·8″). Of nine adult subjects who did two hours of mental multiplication, five felt 'medium' at the end of the period and four felt 'tired.' Both before and after this two hours of work nonsense syllables were memorised. For the five who felt 'medium' after work "the average ratio which the average number of nonsense syllables remembered *after* is to that *before*" is 101·2 (A.D.=3·1), while for the four who felt 'tired' it is only 80·6 (A.D. = 14·9). In this case correlating change of feeling for the worse with decreased efficiency at the end of the work period gave a coefficient of + ·99. Finally, for twelve adult subjects, decrease of efficiency in mental

II. EXPERIMENTAL DATA.

The question concerned in obtaining the data given here was whether the 'fatigue' feelings of a group of workers vary in any characteristic way throughout a day's work. That they would do so might seem to be obvious, but it is by no means obvious just what variations they would show. Attempts were therefore made to obtain records of their 'feelings' from two groups of workers at regular intervals during each of several days. The 'feelings' concerned were what would be regarded generally as 'feelings of fatigue.' No attempt was made to analyse them minutely. The subjects were asked merely to state whether or not they were experiencing certain degrees of 'fatigue' feelings at certain periods throughout the day.

A. Data obtained from a group of twenty women medical students.

In the carrying out of an experiment dealing with the relation of certain mental tests to mental 'fatigue[1],' a group of twenty women medical students[2] was given three tests (lasting altogether about four minutes) once an hour; the first test period on any day being at 10 a.m. and the last at 5 p.m. Thus, there were 8 test periods in a day, any two separated by approximately an hour; and in the intervals between the test periods (and also for an hour before the first) the subjects carried on their regular academic work—attendance at lectures, demonstrations,

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multiplication (as a result of two hours work) was correlated with fatigue feelings and yielded a coefficient of + ·37 (*ibid.* 72–79).

G. E. Phillips, also (*Mental Fatigue*, 65–66) presents data bearing upon this point. He classified fatigue feelings as 'Not tired at all,' 'Slightly tired,' 'Fairly tired,' 'Tired,' and 'Very tired,' and correlated these feelings with depreciation in multiplication and cancellation tests during ninety minutes of continuous work with these tests. The correlation coefficient between feeling tired and depreciation in multiplication in these conditions was ·42 (p.e. = ·086), though between feeling tired and cancellation it was not significant. The subjects were 35 boys between the ages of 11¼ and 13¼. These subjects however were working at maximal effort and were doing their best to disregard fatigue feelings—which indeed was also true of Arai's subjects.

[1] See This *Journal*, 1920, x. 327–344.

[2] These students were taking the medical courses at the London Royal Free Hospital School of Medicine for Women.

etc. The experiment lasted for 5 days. The work in this case was exclusively 'mental,' and the question here to be considered was whether the 'fatigue' feelings varied in any characteristic way during a day of such work. The subjects were therefore required, before starting the tests at each test period, to record in writing on the cover of the test leaflets their feelings of relative 'fitness.' The printed instructions were:

Underline the word or words best corresponding to your feelings.

Fit.
Fairly Fit.
A Little Tired.
Very Tired.

The subjects (who were very trustworthy) were of course instructed to make their records as exact as they could, and it is fairly certain that they did. It was part of the plan of the experiment that none of the subjects was to discuss it or anything connected with it with anyone while it lasted. As regards the 'feelings of fatigue,' therefore, no subject knew what any other subject had recorded while the experiment was in progress.

In order to be able to compare these feelings at different times of the day, numerical values were arbitrarily assigned to each of the four degrees of felt fitness recorded. 'Fit' was given the value 4, 'Fairly Fit' the value 3, 'A little Tired' the value 2, and 'Very Tired' the value 1. The 'fatigue' feelings of the whole 20 subjects at any test period could thus be expressed numerically.

The question of course arises how far the ratio of the numbers 4, 3, 2 and 1 expresses the relations between the four degrees of 'fatigue' feelings; but to answer this question one would require a 'hedonic calculus'—indeed, a 'hedonic calculus' for each subject. Still, there is no doubt that 'Fit' indicates an experience more pleasantly-toned than that indicated by 'Fairly Fit,' and similarly that the experience indicated by 'Fairly Fit' is more pleasantly toned than that indicated by 'A little Tired,' and so on. But whether the hedonic difference between the experiences indicated by 'Fit' and 'Fairly Fit' is identical with that between the experiences indicated, for instance, by 'Fairly Fit' and 'A little Tired,' is a question to which no given answer could be accepted as absolutely accurate. It is therefore assumed that the values here assigned to the four degrees of 'fatigue' feelings are sufficiently indicative of their differences to give a valid idea of diurnal changes in these feelings.

In Table I and Fig. 1 the results are given for the last four days of the experiment, the first day being omitted as an adjustment period.

The *average* feeling is employed for comparative purposes. Thus, the number given as the 'Mean' at any period in Table I is obtained by the following formula: (No. of subjects recording themselves 'Fit' × 4 + No. recording themselves 'Fairly Fit' × 3 + No. recording themselves 'A little Tired' × 2 + No. recording themselves 'Very Tired' × 1) ÷ the total number of subjects (20). For instance, on Wednesday of the experiments (see Table I) at 10 a.m., 8 of the 20 subjects recorded themselves as 'Fit,' 6 as 'Fairly Fit' and 6 as 'A little Tired.' Hence, we have

$$\{(8 \times 4) + (6 \times 3) + (6 \times 2)\} \div 20,$$

which gives 3·1 as the numerical expression of the *mean* 'fatigue' feeling at that period. It will be observed that the larger this number, or the

Table I.

Numerical expression of diurnal changes in the feelings of weariness of 20 women medical students engaged in academic work.

		Hour of Day							
		10 a.m.	11 a.m.	12 (noon)	1 p.m.	2 p.m.	3 p.m.	4 p.m.	5 p.m.
Tuesday	Mean	3·22	3·21	3·10	3·00	3·10	2·90	2·42	2·42
	M.V.	·69	·66	·66	·63	·66	·66	1·01	·59
Wednesday	Mean	3·10	3·15	2·95	2·75	3·10	2·90	2·75	2·40
	M.V.	·72	·76	·73	·52	·63	·63	·60	·56
Thursday	Mean	3·28	3·40	3·44	3·12	3·12	2·60	2·20	2·40
	M.V.	·56	·54	·44	·40	·33	·66	·44	·87
Friday	Mean	3·44	3·33	2·83	2·60	3·00	2·80	2·44	2·44
	M.V.	·68	·67	·46	·63	·44	·64	·88	·88

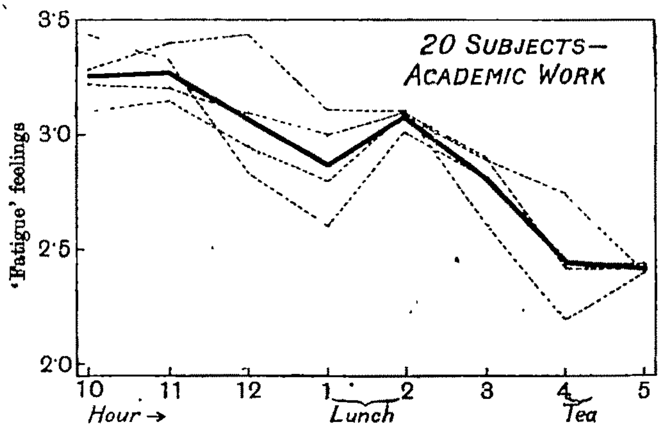

Fig. 1. The broken lines are the curves for each of the four days; the solid line is the average of the four daily curves.

higher the curve in Fig. 1, the more pleasantly toned are the activity feelings of the group.

Lunch was taken between 1 p.m. and 2 p.m., and tea, occupying about twenty minutes, soon after the 4 p.m. test period.

B. Data obtained from a group of fifteen young women typists.

The data now to be described were obtained in a typewriting office from a group of fifteen young women typists. The working conditions in the office were good: physical features, such as lighting and ventilation, were satisfactory, and the personal relations between the management and the employees were excellent. Hours of work were 7¼ per day, except on Thursdays when they were 4. The morning spell was from 9 a.m. until 1 p.m. without any regular break, though an interval for refreshment was allowed about half-way through the spell if it were desired. After lunch work was resumed at 2.30 p.m. and ceased at 6 p.m., there being a break of fifteen minutes for tea at 4.30 p.m. Wages were on a weekly time basis.

The work in the office consisted mostly of typing from MSS. but involved as well certain clerical duties other than typing.

During three days in one week (Tuesday, Wednesday and Saturday) this group of fifteen typists recorded at intervals their feelings of relative weariness. Blanks for recording these feelings were distributed to them at the times when the records were desired: the character of the blanks is shown below. It was intended that these should be filled out 10 times during the day; but it seemed desirable that the distribution and collection of them should be made by one of the office employees rather than by a stranger, and this resulted in their not being distributed at all of the times originally determined upon. On two of the days they were distributed 8 times only.

The Record Blank.

FEELINGS OR FITNESS OR TIREDNESS

Underline the word or words *which best describe your feelings* at this period.

1. DEGREES OF TIREDNESS.
   Fit (not tired).
   A Little Tired (generally, or in some special part).
   Tired (generally, or in some special part).
   Very Tired (generally, or in some special part).

Note also, *by underlining,* whether you have any *aches, pains, or feelings of tiredness* in any of the following parts of the body: Eyes, Back of neck, Head, Shoulders, Arms or wrists, Sides or back, Legs.

2. OTHER FEELINGS.
   Feeling of *clumsiness* (awkwardness).
   Feeling of *boredom* (small or great).
   Feeling of *excitement*.
   Feeling of *tension* (strain).
   Feeling of *hunger*.

Note any other feelings you may have..

Name..............................*Hour*.................................

General character of last hour's work ....

The subjects, who were naturally not trained in self-observation, were very carefully instructed to record as exactly as possible just how they *felt* at the various times, and to add to the blank if it did not provide a sufficient range of feelings. In some instances additions were made; *e.g.* 'pressure on head' was written in several times by one typist. The fact that a number of alternatives was presented in the blank would operate against suggestion. It was made clear to the subjects that the records would be treated confidentially, and it was requested that each subject should preserve absolute secrecy concerning her records until all the data were collected. On the whole, therefore, these records seem as reliable as any that could be obtained from subjects not specially trained in introspection.

For the purpose of comparing the feelings at different times of the day, the data were treated in the same way as the data obtained from the group of medical students; the only difference being that an attempt was here made to make finer distinctions between various weariness feelings. This difference is indicated by the numerical values here assigned to the different feelings recorded, which were as follows: 'Fit' = 4; 'A little Tired, in some special part' = 3; 'A little Tired, generally' = 2·5; 'Tired, in some special part' = 2; 'Tired, generally' = 1·5; 'Very tired, in some special part' = 1; 'Very Tired, generally' = 0. For comparative purposes, *average* feelings were used as before, the method of obtaining them being identical with that explained in connexion with the former group. The results are shown in Table II and Fig. 2, which are to be read in the same way as Table I and Fig. 1.

The records made in the second part (2) of the blank were relatively few and unimportant; the results given below are therefore concerned altogether with the feelings recorded in the first part (1) of the blank.

The detailed records of 'aches, pains, or feelings of tiredness' (see blank) in special parts of the body give concrete feeling to these generalised results. The parts of the body mentioned most frequently are the eyes, the head, the neck and the shoulders. The individual records tend to become extensive as a spell of work proceeds, and towards its end assume a form showing clearly the predominance in the experience of the workers of unpleasant feeling-tone. The following is the record of one typist towards the end of a morning's work: 'Tired generally; in

TABLE II.

Numerical expression of diurnal changes in the feelings of weariness of 15 *young women typists.*

	HOUR OF DAY (*Tuesday*)									
	9.15 a.m.	10.15 a.m.	11.10 a.m.	12.10 p.m.	1 p.m.	2.35 p.m.	3.35 p.m.	4.30 p.m.	5.15 p.m.	6 p.m.
Mean	4·00	3·73	3·58	3·00	2·93	3·85	3·44	2·90	2·72	2·61
M V.	·00	·39	·60	·54	·40	·27	·60	·50	1·00	·70

	HOUR OF DAY (*Wednesday*)							
	10.30 a.m.	11.15 a.m.	12 (noon)	12.50 p.m.	2.40 p.m.	3.45 p.m.	4.50 p.m.	5.50 p.m.
Mean	3·37	3·25	2·77	2·27	3·65	2·94	2·82	2·30
M.V.	·78	·57	·62	·53	·56	·50	·74	·84

	HOUR OF DAY (*Saturday*)							
	9.15 a.m.	10·30 a.m.	11·30 a.m.	12 (noon)	12.50 p.m.	2.30 p.m.	3.30 p.m.	6 p.m.
Mean	3·72	3·50	3·40	3·05	2·88	3·67	3·05	2·58
M.V.	·42	·64	·70	·55	·79	·50	·61	1·07

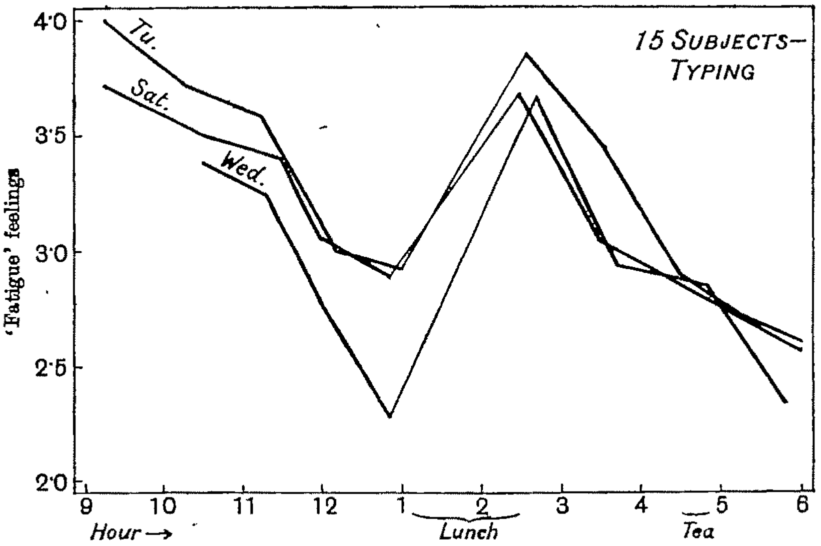

Fig. 2. Each curve represents one day.

head, eyes, neck, wrists; hungry[1], bored, feeling of strain.' The records make it clear that as work proceeds the area of the body in which unpleasantly toned sensations are localised becomes wider and wider.

III. DISCUSSION.

The main tendency of the data obtained from both medical students and typists is perfectly definite[2], and might be expressed by saying that during continuous work feeling-tone, in so far as it is dependent upon 'feelings of fatigue,' tends to become more and more unpleasant. This conclusion would probably be accepted as obvious; and it is not implied that the results given above were necessary to establish it. These results have been presented in order to give definite expression to common experience concerning the relation of continuous activity to feeling-tone, and thus to make possible a comparison of diurnal variations in feeling-tone during work with diurnal fluctuations in industrial output.

Such a comparison shows at once that the normal industrial output curve and the curve for feeling-tone during work are in certain respects strikingly similar. At the same time they are in other respects different. Thus, whatever the class of work, output tends to fall off in quantity towards the end of a spell, and to rise again after the mid-day interval. In these respects the output and feeling-tone curves are so nearly the same that one might reasonably suppose *either* that these variations in output are directly due to the variations in feeling-tone *or* that both sets of variations are due to variations in some third factor—a physiological factor. On the other hand, with most classes of work output *tends to improve* over the first part of the morning spell; and while there is some evidence of a similar improvement in feeling-tone in the results obtained from the medical students, there is no evidence at all of such improvement in the results obtained from the typists.

Now the normal response to the feelings of weariness is to slacken effort or to cease work altogether. This response occurs independently of any deliberate intention of effecting a decrease in the unpleasantness of the feeling by means of it, although normally its occurrence produces this result immediately[3]. The smith who swings his hammer until every movement of his arm causes an 'ache' relieves himself at once of this unpleasant sensation by simply ceasing to hammer. Such facts as this explain the universal *belief* that relaxation of effort lessens the feeling of weariness; and if this belief be taken in conjunction with the fact that the normal *involuntary* response to the feelings of weariness is to slacken effort or to cease work altogether, we may be certain that where such feelings occur work will be slowed down, and will be slowed down the more the greater the intensity of these feelings. There can be no doubt what the normal response would be to 'Tired generally; in head, eyes, neck, wrists; hungry, bored, feeling of strain.' Have we not here the explanation of decreased output towards the end of a spell of work?

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[1] In the last two recording periods of the morning, hunger is frequently mentioned.

[2] The curves given here are of the same character as those of Thorndike (*J. of Applied Psychol.* I. 3, 266–267) for the variations in 'satisfyingness' felt during 2 and 4 hours work at grading compositions. Thorndike had 29 adult subjects.

[3] Cf. Ward's *Principles of Psychology*, ch. II, § 5. On p. 55 it is said that if a "painful presentation is a movement, it is suspended."

It may be argued that such an explanation is at any rate not complete because the feelings of weariness with their unpleasant feeling-tone will themselves be determined by physiological conditions, and that a complete explanation would need to determine the precise nature of these conditions. This is certainly not denied. But what it seems reasonable to deny is that the physiological conditions concerned, whatever they might be, would have anything to do with output except as affecting feeling-tone; for it is this which seems to stand in a direct dynamic relation to the will of the worker[1]. Without entering further into a discussion of what is or is not a complete 'explanation' here, it may be said that the character of the feeling-tone towards the end of a spell of work *should* affect output in precisely the way it is found to be affected; and it may be concluded that any explanation that neglects feeling-tone is incomplete.

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[1] Cf. Thorndike, *Educational Psychology*, III. 124–125: "The behaviour which results in certain products such as sums done...is subject to the laws of all behaviour, and to no others. If a continuance of the productive responses at the same speed and in such form as to give equal quality, is satisfying to the individual concerned, he will continue them. If such continuance brings discomfort, he will tend to stop them altogether, or to intermit them, or to make them in such altered form and speed as lets them bring relative satisfaction. Stopping the work outright does not of course occur in the great majority of experimental investigations of fatigue, but is very common in ordinary mental work." And Joteyko writes (*The Science of Labour*, 20–21): "The psychic factor which regulates the expenditure of energy of the human motor...is the sensation of fatigue...Pain and fatigue are the physical and psychical conditions which we are endeavouring to avoid throughout our lives."

In Thorndike's valuable discussion of 'fatigue' what is emphasised are the normal responses to the 'satisfyingness' or 'unsatisfyingness' of continuing at work. These opposite 'feelings' are of course very complex and arise from a variety of conditions. Thus, for instance, the 'unsatisfyingness' of work may be due primarily to the fact that continuance at work involves the temporary repression of various social impulses, or simply to the fact that it is producing sensory aches and pains. In the present paper the 'satisfyingness' of continuing at work is considered only in so far as it is dependent upon 'fatigue' feelings.

It may be noted that where continuing at work involves the inhibition of strong impulses the total work of the organism is not at all measured by what is ordinarily called the product or output of work. The internal work involved in such inhibition of impulses gives no obvious indication of its occurrence, though its effects may be serious.

If this view is correct, it is necessary to try to explain why the curves for the 'fatigue' feelings and for industrial output are not in agreement throughout; why, that is, if these feelings explain or help to explain certain features of output curves, they do not explain all features. In particular, why should output improve during the early part of the day, since, assuming the data given above to be reliable and typical, the 'feelings' of fatigue apparently become more intense almost from the very beginning of work?—It would seem that the explanation of this fact should be in terms of the operation of incentives and warming up.

The incentives to much modern industrial work are certainly not exclusively the feelings of pleasure derived from the mere performance of it. Indeed, such work is frequently so confined and specialised that its performance can yield nothing very satisfying in itself; nothing comparable, for instance, to the pleasurable cœnaesthesis resulting from the moderate or even vigorous exercise of the whole body. When these conditions obtain, work is carried on because of incentives (including habits) extraneous to the work itself. In so far as the tendencies excited by these incentives are being realised through the continuance of the work, the feeling-tone of the worker will be to some degree pleasurable, and thus will contribute to the further continuance of the work. As the 'feelings of fatigue' become more definite, the worker will oscillate between the two responses that are characteristic of pleasurable and painful feeling-tone respectively: if he attends to the pleasurable feeling-tone aroused by the knowledge that he is accomplishing his ends, he will tend to continue his work without relaxation of effort, whereas if he attends to the 'feelings of fatigue' with their painful feeling-tone he will tend to relax his effort. Extraneous incentives will operate more or less effectively so long as the mere performance of the work itself does not become obviously painful. Consequently, although feeling-tone, in so far as it is dependent upon 'fatigue' feelings, may become more and more unpleasant from the very beginning of a spell of work, effort may not be relaxed until its painfulness reaches a certain degree of intensity. Of course, these feelings may be altogether disregarded by the worker, as they often have been under experimental conditions; but normally they are not. The degree of intensity which they must attain before they give rise to a slackening of effort is doubtless different for different persons and for the same person at different times, but it will generally be directly related to the strength of incentives.

The pleasurable feeling-tone arising from the sense of realising tendencies excited by incentives extraneous to the work will not, however, explain the early actual improvement in output (during a day's work); all it does is to provide a reason for maintaining for some time the working situation and would thus only seem to guarantee that output would not actually deteriorate from the very beginning of a spell of work. To explain the actual improvement in output in this early stage, recourse must probably be had to the physiological factors (and perhaps psychological[1] factors also) forming the basis of warming up, the 'facilitation' due to which would more than compensate for any 'inhibition' due to 'fatigue' feelings of mild intensity. Granting such factors, the cause of the divergence between the output and 'fatigue' feelings curves in the early part of a spell of work seems clear enough.

While there can be no doubt what will be the effects of the feelings of weariness (including local aches due to the maintenance of one posture or one set of movements over long periods) upon immediate effort and output, at least when these feelings have attained a certain intensity, their more remote effects upon the preservation of health are quite unknown. They may be a preventive of over-work, or, as Thorndike[2] suggests, they may be a useless by-product of activity to which hygiene need pay no attention—a "mere accident" of activity which "habitual disregard may abolish," or suppress. The field for research here is certainly an interesting one, especially in view of the widespread belief that the seeking and avoiding reactions characteristic of pleasure and pain are of profound biological significance.

IV. SUMMARY.

The object of this paper has been to suggest the importance of considering certain of the subjective phenomena of 'fatigue' when dealing with the characteristics of normal work. Curves representing diurnal variations in the 'feelings of fatigue' caused by work were given, and these show certain striking resemblances to common industrial output curves. It is strongly suggested that the deterioration of output towards the end of a spell of work is a direct response to the painful feeling-tone of these 'feelings of fatigue.' These feelings may thus play a far greater part in normal work than current 'fatigue' investigation, engrossed with physiological problems, recognises. And it is suggested that 'feelings' generally—and not merely 'fatigue' feelings—should be accorded more notice in connexion with various sides of industrial life than they at present receive.

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[1] Cf. Thorndike, *Educational Psychology*, III. 76: "The notion of Warming-Up...has included the diminishing annoyance of a process due to the progressive inattention to competing desires, interest in achievement, and the like."

[2] *Psychol. Rev.* 1900, VII. 482.

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THE INTELLECTUAL RESPECTABILITY OF MUSCULAR SKILL[1].

BY T. H. PEAR.

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I. *What is intellect?* (pp. 163, 164).

II. *Some problems stated* (pp. 164, 165).

III. *Some characteristics of kinaesthesis* (pp. 165–172):

  (*a*) *The loose connexion of kinaesthesis with language* (pp. 165–167).

  (*b*) *The significance of individual differences in kinaesthetic imagery* (pp. 168–172).

IV. *The 'intolerance' of persons with predominant kinds of imagery. The utility of visual imagery in learning muscular co-ordinations* (pp. 173–175).

V. *Is a 'language of kinaesthesis' possible?* (pp. 175–177).

VI. *The importance of motion-study in this connexion* (pp. 177, 178).

VII. *Improvement of the social and intellectual status of kinaesthetic knowledge* (pp. 178, 179).

VIII. *Some further speculations* (p. 180).

I. WHAT IS INTELLECT?

THE word intellect has been used in many senses, of which at least two are important in this connexion[2]. It may cover all forms of cognitive events, in which case intellection becomes merely the equivalent of cognition. On the other hand, it may designate the processes of elaboration which the mind applies to its material, and in particular that kind of elaboration which leads to conceptual thinking. The point of view which this paper seeks to justify is that, in both these senses of the word 'intellect,' muscular skill has a higher intellectual value than is usually assigned to it, and that this value is susceptible of being increased considerably.

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[1] The substance of this paper was read to the British Psychological Society on 12 March, 1921.

[2] Cf. Baldwin's *Dictionary of Philosophy and Psychology*.

We may first inquire whether such skill can be justifiably described as intellectual.

Bodily skill, or the ability to deal with the world by means of one's muscles, joints and tendons, carries with it a specific and unique kinaesthetic knowledge. This knowledge can obviously be called intellectual in the first sense mentioned above. Yet it is as incapable of perfect translation into the terms of another sense as is music into colours or words, for as we shall see, it possesses scarcely any words in its own right. Many persons therefore require to make a mental effort in order to realise that, in spite of its comparative wordlessness, kinaesthetic knowledge may form a basis for conceptual thinking, thereby establishing a primary claim to be called intellectual in the second sense of the term. The peculiar nature of such knowledge, however, raises certain difficulties when it is dealt with from the psychological aspect. These will be described later.

Kinaesthesis is usually accorded scant respect by the world of intellectuals, as we know them today. Classed with the 'lower' senses, it is contrasted with the 'higher' senses of sight and hearing[1]. Though such a division seems justified at present its utility is possibly over-estimated[2].

II. Some Problems Stated.

It is now possible to suggest a number of questions which arise from these considerations. Some attempt at answering them will be made in this paper.

(1) Do not most educated persons regard the intellectual value of kinaesthesis as slight?

(2) Is such an attitude justified, in the light (*a*) of primitive man's early development[3], (*b*) of certain present tendencies in social behaviour?

(3) What are the chief factors which have brought about this attitude?

(4) Is the discovery of these factors likely to help in predicting future developments in man's mentality?

(5) Is an intellect, the foundations of which do not include contributions from the 'higher' senses of sight and hearing, necessarily inferior

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[1] Cf. E. B. Titchener, *Textbook of Psychology* New York, 1911, 114 f.

[2] Perhaps Professor Nunn feels this when he writes kindly of the 'humbler' senses. Quoting Miss Margaret McMillan, who says "The patience of the poor is not all patience. It is largely insensibility," he adds, "To such children a shower-bath, with its powerful appeal to dull senses and flaccid muscles, may mean a veritable beginning of intellectual and moral enlightenment" (*Education, its Data and First Principles*, London, 1920, 164).

[3] The chapters on "Tactile and Motor Impressions," in F. Wood-Jones's *Arboreal Man* (London, 1918, 157–173), are full of suggestions upon this subject.

The ludicrous, too, is felt by her in "the bulge of water-melons, the puffed-up rotundities of squashes." But her amazing insight—the word is not permissible here, but we have no other—into realms which seem to many to be explorable only through sight and hearing, may easily distract attention from an important consideration. Helen Keller was educated by and obtained her language from persons whose knowledge of their own world had been received chiefly through the channels of sight and hearing. It seems clear, therefore, that the terms of her language, while often merely analogical, are just as often almost meaningless if regarded as descriptions of fact. The words 'ruddy' and 'dissonance'; the statement "I know how budding trees look" can have no direct sensous meaning for her[1].

Perhaps we may be allowed—though it seems scarcely permissible—for one brief moment to adopt towards this life-history the detached attitude of the scientist. At once a speculation forces itself forward. Only by the devoted efforts of her teacher and friends was her mental development made possible; yet to what extent did this assistance cause her mind to deviate from the path which we may imagine that it might have taken for itself, under impossibly ideal circumstances of protection and companionship?

Let us suppose for a moment that, while being sheltered from the rigours of life, she had developed ideas both simple and complex about her world chiefly upon the basis of kinaesthesis, and with no reference whatever to sight or hearing. We may now further imagine her, having constructed a mental world quite different in quality from that of the ordinary person, to have been entrusted completely and exclusively with the bringing-up of another child, also lacking vision and hearing, who, in turn, had become the educator of still another. Gradually these references to sight and hearing, as analogical as our own attempts to picture the waves of wireless telegraphy, would have faded away, leaving a structure of knowledge which would be the unique possession of the congenitally blind-deaf; unique not only in the nature of its fundamental sensation material, but also in the degree of perfection attained by its functions. Moreover, in many of those adjustments to environment in which these attainments entered into competition with those of any ordinary human being, they would have proved to be superior.

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[1] "....a venturesome spirit impels me to use words of sight and sound whose meaning I can guess only from analogy and fancy" (p. 45).

(*b*) *The significance of individual differences in kinaesthetic imagery.*

The evidence derived both from the comparison of the introspection of different individuals, and from experiment, makes it clear that the kinaesthetic imagery of different persons is characterised by very considerable variations. Some people appear to possess kinaesthetic imagery of a degree of excellence comparable with the common cases of well-developed visual memory. Yet up to the present there have been few to collect the details of such gifts. The earlier *questionnaires* gave their answerers comparatively little opportunity to record this kind of imagery. Even if they had done so, the 'motile'—to use a somewhat old-fashioned phrase—would have found it difficult to express himself in words, except of course when describing the processes of speech or writing. Important as these inquiries undoubtedly are, they teach us almost nothing concerning the mental life of the 'muscular man,' and much more investigation is needed into the kinaesthesis of the larger musculature and of the body as a whole.

Until more is known about the characteristic individual differences in kinaesthetic memory, we must content ourselves with speculations based upon other spheres of imagery, aided by our casual self-knowledge. With this warning in mind, the somewhat bold speculation may be made, that kinaesthetic memories may differ in their vividness, clearness, 'constancy,' or unchangeableness, readiness for use and ability to carry meaning.

The task of assessing all these characteristics would not be easy. It is obviously more difficult than in the spheres of sight or hearing. For the person who describes visual imagery may command all the subtle shades of significance with which centuries of language and literature have provided him; for the recorder of auditory memory the whole notation of music and the rich resources of physics are ready to help him to hammer out as thinly as possible, and then to nail down, the edges of his meanings. But where is the notation of action; where shall we seek for the grammar, the syntax and the theory of harmony of bodily movement? Perhaps some beginnings of them lie in the basal ideas, many of them as yet scarcely explicit, of manual training, of eurhythmics, of folk-dancing and of 'motion study.' Certainly their material lies tantalisingly hidden in the minds and bodies of Anna Pavlova, of Annette Kellermann, of C. B. Fry and W. T. Tilden. But whether these happy mortals, when they move, are making poetry without knowing it; how far their perfection is the result of hard-won synthesis and to what extent it is a delicate natural polish which self-analysis would crack; of these things we—and possibly they—know almost nothing.

But while we marvel delightedly at the grace of their movements, perhaps feeling in ourselves the while feeble and faint tendencies to copy them, the sight suggests another psychological question. May the pre-eminence of these persons be closely connected with their ability, on seeing a new movement, to register and subsequently to recall it directly through their kinaesthesis, and not through the aid of any intermediate imagery, like that of sight? May it be that the number, vividness, clarity, readiness and meaning-carrying capacity of their kinaesthetic images enable them to learn a new movement in swimming, dancing, fencing or skating as easily as a first-class visualiser may take in details of a picture after one brief glimpse, or as Mozart wrote down from memory the *Miserere* of the Sistine Chapel after hearing it twice?

This seems to be possible, for before a new co-ordination of movements can be learnt by most ordinary people they find it necessary to overcome something very like a pronounced physical and mental resistance against placing their limbs or body in this particular position. They feel as if, in order that the new movement shall be learnt, it were necessary [illegible] blast a path through some stratum of high resistance in the neuro-[illegible]uscular system. Often, moreover, once this newly taken-up bodily [illegible]on has been abandoned, many unfortunates can evoke no genuinely [illegible]thetic memory of it, and so, in order for it to be learnt, it has to [illegible]umed a wearying number of times. As a stepping-stone to these achievements they resort to visual or verbal aids, or use both [illegible]er; they picture the arm or leg in a certain position before putting [illegible]e, or they anxiously mutter, quietly or audibly, some exhortation [illegible] by their teacher, "follow through," "watch the side-line," "bend [illegible] knee," "right shoulder forward."

It is almost certain, however, that at least some persons, endowed with well-marked kinaesthesis, remember their new movement from the very beginning in the same 'language' in which they will eventually wish to express it, in the wordless language of kinaesthesis. And in doing so they ensure their greater success in more than one way. Obviously they save their own and the instructor's time. But loss of time is not the only annoying feature of a *bureau de change*; the value of the foreign currency with which one emerges is always less than that paid in, by the amount charged for changing. And this depreciation of meaning, when knowledge gained in one sense-sphere is translated into the language of another—so vividly realised when one reads the 'meaning' of a great symphony which a concert-programme description vainly attempts to convey, or hears the musical 'representation' of a sea-scene—is at least as great in the case before us.

In order to illustrate these and other facts, some examples from my own experience may perhaps be admissible. By way of preface, I will mention that the variety, range, readiness and plasticity of my visual imagery seem to be inversely matched by the paucity, narrowness and 'stickiness'[1] of my kinaesthetic equipment[2]. The following observations are taken from my notebook, with a very few minor corrections of obscurities due to the clumsy wording of notes written hastily:

> At noon to-day I finished an elementary lesson in figure-skating, during which I had found considerable difficulty in getting the 'feel' of the various muscular coordinations—chiefly concerned with feet, knees and hips—as described and exemplified by the instructor. At 6.30 the same evening, while writing these notes, I tried to recall, in kinaesthetic imagery, the morning's lesson on the 'back outside edge.' In spite of all my efforts, the recall persistently came in the following terms:
>
> A very clear visual image of the instructor, seen facing me at the distance of a foot. The clearest part of the image was the top of his head, the next clearest his face and his smile, and the upper part of his body. The image pushed itself into consciousness almost to the exclusion of everything else, and the utter inability of such an experience to assist, in any way, a person learning to skate is too obvious to req[illegible] further emphasis.
>
> With an effort on my part there then came another visual image of the instructo[illegible] skating-boot executing a back outside edge. This boot, if not firmly held in [illegible] began to focus itself into details such as the pattern of lacing, scratches, [illegible] occasional patch on the side. At no time did even a visual image of my ow[illegible] come into consciousness, probably for the very good reason that I had been for[illegible] by the instructor to look at it.
>
> The only kinaesthetic imagery which could be recovered at this time (i.e. 6[illegible] after the lesson) was a very faint and feeble image of scratching the ice with [illegible] of the skate; i.e. an image of a movement which I had been expressly fo[illegible] to make.
>
> At another time it was recorded that "any number of visual images of th[illegible] structor in any position can be obtained and these must be re-translated mirror-w[illegible] into visual images of my own body before they are of any use."

A further extract from notes written on the same evening as those first quoted may make another point clear.

> In trying to remember a particular skating movement which involves keeping the right shoulder back, I 'see' my own shoulders much more easily than I can

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[1] This characteristic is described on pp. 171–2.

[2] How far such lopsidedness of imagery development is attributable to heredity, to environment or to both is a psychological question which often arises in a practical form. It is usually ignored or begged.

'feel' them in the right position. I visualise quite clearly my right shoulder when it is held back, including even the part of the (brown) coat which covers it, quite regardless of the anatomical impossibility of seeing my right shoulder when my eyes are looking over my left. When the instructor shows me how to execute a new movement, I am naturally inclined merely to admire him passively as a moving picture (after one evening's skating I had continual visual images of him in all kinds of skating positions before I went to sleep) but, unless I force myself to do so, I get no muscular imagery while I am watching him demonstrate a movement.

How then do I learn to skate at all? In so far as any progress is made, it appears to be[1] through visualising myself and others performing the movement in question. The images are very clear though not vivid; they are like a clear well-taken carbon photograph, or like a sketch made with an H.B. pencil, and therefore gray. When taking a lesson I tend to look on at the whole performance as a passive spectator, just as if I were watching a stage dancer, and only occasionally do I remind myself that it is my lesson, and that therefore I must do something more than merely looking on.

It is very easy for me to attend to the look of the performance; it is correspondingly hard to attend to the 'feel' of it[2]; it is as if the nervous energy had to blast its way through some resistive stratum of the brain. This illustration of blasting one's way is perhaps supported by the fact that the 'feel' of a new movement, when it does arrive, often comes quite suddenly. Perhaps Dr Montessori may be describing some [illegible]ilar experience when she writes of the "writing explosion"; the sudden discovery, [illegible]ttended with great excitement, by her little pupils that they are able to write.

In learning a dance there is, too, the same tendency to attend to the look and not [illegible]o the 'feel' of it. In order to learn any new dance I must go behind the demonstrator [illegible]d see the position of the person's feet, just as if they were my own. I must then [illegible] my feet in the same position (imaged visually if the demonstrator is absent) [illegible]ing very carefully to the 'feel' of the position.

[illegible]stly, I might mention that though my muscular memory for movement is so vague, unready and 'sticky,' so to speak, my sense of balance appears to be [illegible], e.g. I have noticed that keeping my balance in a new position, or deliberately [illegible] from a new position, presents few difficulties to me.

[illegible]ould not be surprised to discover that the apparent paucity of my kinaesthetic images is due, to a considerable extent, to their 'stickiness' or general agglutination, by which words I mean to indicate:

(*a*) The lack of clear-cut differentiation between different combinations of imaged movements, especially if they are somewhat similar. It seems to me, as a visualiser, comparable to the lack of differentiation which would exist in the visual imagery of an uneducated person, attempting to recall, let us say, a series of differently shaped human skulls which he had seen; while in the imagery of a visualising anthropologist these distinctions would be prominent. I imagine that in a first class athlete, especially in one who plays well several somewhat similar games, e.g. tennis, lawn tennis and badminton, this differentiation (which of course, is one of the first necessities for thinking) has gone on to a much greater extent than in myself. Perhaps it is comparable to a good actor's power of speaking the same sentence successively in several different dialects of the same language.

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[1] (Note added subsequently): "Or rather, 'one important factor appears to be.'"

[2] This reminds one of the fact that, on injury to the post-Rolandic convolutions of the brain, a patient may lose his power of attending to his touch sensations while retaining that of attending to everything else. Might it be that in the case of the person whose kinaesthetic imagery is undeveloped some corresponding part of the brain has never been properly 'opened up'? (Cf. footnote [2] on page 172).

(*b*) The fact that my kinaesthetic images seem—if it is not unsafe to use a pictorial metaphor—difficult to coax out of their holes. Perhaps it is that they are not only faint, but extremely fleeting (like the visual images which Dr Rivers describes as forming part of his experience when awake[1]), and that only after many efforts can one get a grip on them.

These examples[2] may help the reader whose imagery tends predominantly towards the visual or the kinaesthetic type to realise the great difference between the kinds of mental apparatus with which different persons may attempt the same task. The utter irrelevance and futility of many of the visual images recorded in the notes are worthy of special attention. Some of these images were not only useless but their presence may have obstructed any incipient attempts to learn in the direct way. For example, visualising the upper part of the instructor's body seems scarcely likely to have assisted in any way whatever; visualising one's own skating-boot may have been a little more helpful, though th[illegible] doubtful.

It is, however, impossible to be satisfied with any simple belief that visualisation is valueless in learning muscular coordinations. Many visil[illegible] cherish those diagrams and pictures which, though they are some[illegible] execrable, both in conception and in execution, often form almos[illegible] only intelligible information in many books which profess to des[illegible] how to play any particular game. After a glance at such illustr[illegible] the visile may carry away, once and for all, the gist of a wordy ex[illegible]tion which may never have been clear even to its writer.

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[1] *Instinct and the Unconscious*, Cambridge, 1920, p. 11 f.

[2] When I wrote these notes, I was unacquainted with Professor Washburn's book, *Movement and Mental Imagery*. In connexion with my own ease of attending to the 'look,' and difficulty in attending to the 'feel' of a muscular performance, her discussion of the relation of differences in imagery-type to "individual differences in the appeal of certain kinds of stimuli to attention" (p. 43 f.) is helpful, while Professor Wood-Jones's statement (*op. cit.* p. 171): "It is perhaps not beyond possibility that the full lodgment of all pictured movements (his term for kinaesthesis) is not yet permanently effected in all human brains, and that the process is still in progress" is hopeful.

IV. THE 'INTOLERANCE' OF PERSONS WITH PREDOMINANT KINDS OF IMAGERY. THE UTILITY OF VISUAL IMAGERY IN LEARNING MUSCULAR COORDINATIONS.

It is very common to meet with athletes who maintain stoutly that books are of no use in learning a game. If by this be meant merely that books are useless if not supplemented by diligent practice, the remark is so obvious as to be uninteresting. If it means that many existing books on games are bad, the psychologist's interest in it becomes a little keener. But in the mouths of some people the assertion illustrates a most interesting and important psychological phenomenon; the mutual intolerance, arising from ignorance, of people with different types of mentality.

This fact was not missed by Galton, who writes of those men of science to whom visual imagery was unknown:

> They had no more notion of its nature than a colour-blind man, who has not discerned his defect, has of the nature of colour. They had a mental deficiency of which they were unaware, and naturally enough supposed that those who affirmed they possessed it, were romancing[1].

This attitude is often very strikingly shown (though its results and mplications are, probably, not yet sufficiently realised by psychologists d others) by visiles towards motiles, and *vice versâ*. The visile often ds it almost impossible to realise how a motile can ever recall in any rms other than visual images an experience which, it would seem to visile, 'ought' to be naturally recalled in such imagery. The motile milar difficulties of comprehension. (It is more correct to write that difficulties would arise in the improbable event of such thoughts ing to either of these persons.)

For those readers whose predominant imagery does not happen to belong so exclusively to one of these two classes, a few examples may illustrate the importance of this intolerance. Perhaps I may be allowed to begin with the type to which my own mind conforms; the visile. For me visualisation, not only of obviously pictorial experiences, but even of abstract and general meanings, in the form of similes, metaphors, analogies and semi-diagrammatic pictures[2], is so ubiquitous that I frequently forget that many other people do not visualise with the same degree of facility or viciousness. It is unfortunately a somewhat common experience for me to realise, after having lectured to a class for some considerable time, that I have made no attempt to transfer to the blackboard any of the mental diagrams which I have been using. Such outward behaviour might perhaps be interpreted by others as indicating poverty rather than profusion of visual imagery in a lecturer's mind. On the contrary, however, visual imagery of diagrams is so prevalent with me that I have often to remind myself that in order to transmit even an approximately faithful rendering of my meaning to others, I must make my thinking 'visible.'

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[1] *Inquiries into Human Faculty*, London, 1883, p. 85.

[2] There is, usually no *conscious* attempt to picture such meanings. They simply 'are there.' Sometimes they are adequate to the meaning, often they are mere caricatures of it. But like most caricatures they usually cause me to realise with especial intensity some salient point, and not always one which is welcomed.

Many visualisers too, never realise, or refuse to believe that complicated behaviour and thinking may go on in others without the slightest trace of visual imagery. From conversation with some of my friends who have predominant kinaesthetic imagery it seems to me certain that a similar forgetfulness of the peculiarities of others characterises many motiles, especially those for whom kinaesthetic processes,—in particular perhaps, those of language and gesture—seem to be so prominent in their thinking that they even write of the "fiction of visual imagery[1].' In fact, the possibility of any real *rapprochement* of the visile and motile seems at present to be remote, for the following reason. While the visualiser's power to express to another person his experience of visu[illegible] imagery is limited only by his capacity for psychological observation [illegible] for using his mother tongue, which has chiefly been developed to descr[illegible] the seen world, the motile is less fortunate, in that very few of [illegible] characteristic inner experiences[2] are expressible in language at all[3] [illegible] the function of many words relating to bodily movement is me[illegible] describe its visible aspects, seen by others or by its initiator. Some [illegible] moreover, refer less to the bodily movement itself than to that [illegible] instrument moved by the person, or, even more remotely, to some object propelled by this instrument. English out-door games illustrate especially well this tendency for the reference of the term to creep farther and farther away from the initiating limbs. For instance, I take it that such a term as a 'late cut' in cricket may conceivably refer to any or all of the following separate events:

(*a*) the genuinely personal experience; kinaesthetic, visual, tactual, etc., of the batsman;

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[1] The question is discussed in greater detail in this *Journal*, 1920, XI, 1, 77–79.

[2] I venture to call the reader's attention once more to the significance of this word 'inner,' which was emphasised on p. 165.

[3] Or, as a behaviourist might perhaps express it, speech habits are not closely connected with the functioning of the muscles, joints and tendons.

(*b*) the movement of the body and arms and of the bat, before and after it strikes the ball (1) as seen by the batsman, (2) as seen by the spectators;

(*c*) the behaviour of the ball after its impact with the bat.

An indispensable preliminary to the construction of any satisfactory psychology and any really expressive language of kinaesthesis will be a distinct separation in thought of all those events which occur outside the agent's skin from those which take place inside this envelope. Until that is achieved the motile will remain, as he is at present, inarticulate. For of the recognised phrases describing action many bear a spectacular or behaviouristic rather than a psychological meaning, describing the experience of the looker-on, not of the performer, while not a few carry a depressingly complex and unanalysed mixture of both.

It is probably the ability of the motile to recall a new movement n his muscles,' after having performed it but a few times, and con- ected with this, his inability or disinclination to visualise how his body oks when placed in the position required, which leads him to say that ading books on a game is of little use in learning it. If by this is meant t one cannot learn a game without performing the muscular actions hich it requires, it is a platitude. But if, on the other hand, the motile eans that for him photographs, diagrams, and diagrammatic pictures, [illegible] y nothing of the slowed-up cinematographic representation, and the [illegible] mensional wire model of a movement, are of little or no use to him, [illegible] presses very clearly his difference from the visile. For from a really [illegible] gently drawn diagram the latter can often learn in a flash what he [illegible] en trying for weeks to discover from watching an actual demonstra- [illegible] Eventually, of course, he must translate this visual experience into kinaesthetic memories of the movements as they were actually carried out by himself. In this respect therefore, he is at a disadvantage; comparable, perhaps, with that of a person who knows a language but cannot think in it. But whether in the highest flights of any branch of games, the use of a good book does not become essential for rapid progress is still a question well worth asking.

V. IS A 'LANGUAGE OF KINAESTHESIS' POSSIBLE?

We must now inquire whether such a language of kinaesthesis is conceivable, and if so, how it might arise. An attempt will be made to sketch an approach to this problem, and to speculate how, neglecting for the moment practical difficulties, such a language might conceivably be formed.

To the utmost possible degree it should be rid of analogical references to other senses. Sight has one language, hearing has another. Cross-references, such as the colour of a voice or the key of a landscape are, statistically speaking, so rare that they do but call attention to the normal self-sufficiency of these two notations. Kinaesthesis, then, would have to renounce its present bad habit of borrowing terms from the other senses, and botching make-shift tools out of them. Ideally, a visual or auditory term, used in a description of motor experience, would appear unambiguously in its own right.

Supposing it to be possible to furnish descriptive words for different kinaesthetic experiences, it would then be necessary to discover elementary or *quasi*-elementary movements of groups of muscles which are of fundamental and outstanding importance in some branch or branches of industry or physical exercise. Here one can offer only tentative suggestions as to how this might be done. Search might be made among existing manuals of drill and physical training, of eurhythmic exercis and of physical culture systems[1]. Anatomists and physiologists mig give valuable information concerning those important bodily movemen which, structurally and functionally, are relatively simple. In this w there might be isolated from others, so that they could be fairly stric defined, a hundred simple, elementary and typical attitudes of body limbs. Persons could then be taught to take up these positions and again, carefully and attentively, until the descriptive names to them had associated themselves with definite kinaesthetic experi

Such a plan does not seem wildly impossible. In fencing, g cricket, figure-skating—to take only a few cases—definite 'st are learnt. In the last-named exercise, 'new' positions and mov appear to be deliberately composed of definite elementary groups which have been previously given standardised names and consciously realised in experience. All such lessons involve the isolation, from thousands of possible muscular combinations, of some, which for a particular purpose, are 'fundamental.' Perhaps some day, with the assistance of anatomy and physiology, it may be possible to define more strictly the fundamental groups for any special purpose.

Supposing, then, that the important elementary or essential experiences in the world of kinaesthesis had been isolated and labelled, so that agreement exists concerning their meaning; the next step would be to classify them more scientifically—for of course some degree of classification will have taken place already—into categories. Just as in the language of sight there are divisions of words into those representing colours, forms, directions, sizes, etc., so there might be a subjective or psychological classification of man's most important movements. While gross divisions of the movements, corresponding to limbs, fingers, etc., will already have been achieved, it is obvious that some compromise between such a classification and that based upon anatomy and physiology may be necessary. The most useful final grouping might conceivably be guided exclusively by anatomical and physiological considerations.

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[1] The writer's present acquaintance with the Ling and other systems is insufficient to justify any opinion being recorded here concerning the extent to which a scientifically satisfactory beginning has been made in this direction.

At this point the reader will naturally and justly inquire why, if such a badly-needed kinaesthetic language is theoretically possible, it is not already in existence. He may draw the conclusion that there must be some insuperable difficulty which this essay has not taken into account. This may be the case. But, until recently, one valid reason for the non-existence of any satisfactory objective analysis, classification, and standardisation of human movements (and, therefore, of human kinaesthetic experiences) was that usually no two persons could agree not only as to the exact motions which ought to be made for any particular purpose, but even concerning the movements which they themselves had made a moment previously[1]. For most human movements are so very complicated, rapid and individually different that even an unusually favourable combination of natural aptitude and special training in observation cannot ensure a faithful account of them.

VI. THE IMPORTANCE OF 'MOTION-STUDY' IN THIS CONNEXION.

But the last few years have seen a great increase in the number and variety of successful attempts experimentally to record the behaviour both of man and of animals. One of the most interesting branches of this work is that known as 'motion-study.' It has developed, and is still improving, apparatus which, freezing in mid-air, so to speak, the path of any movement, records it permanently by means of a photograph or tri-dimensional wire model, painted in a special way to represent not only the direction of any component movement but also its relative and absolute velocity[2]. Such models will make it impossible in future for there to be two opinions concerning the exact orbit which any limb has described in executing a set of movements.

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[1] Disputes as to whether a particular golfer moved his head at a certain moment are not unknown in club houses. Unfortunately it is quite likely that the person whose evidence is of least value in this connexion is the player himself.

[2] For description and photographs see F. B. Gilbreth, *Motion Study*, New York, 1911; *Fatigue Study*, London, 1916; *Applied Motion Study*, London, 1919. A concise account of motion-study, with illustrations, is given in Chapter I of C. S. Myers's *Mind and Work*, 1920, pp. 2–35.

It seems therefore not inconceivable that as a result of careful motion-study of various occupations there may be built up notations both of objectively seen movements, and of the kinaesthesis corresponding to them. The words used would then refer (*a*) to the objectively demonstrated movements—photographed, cinematographed or 'motion-modelled,' (*b*) to the kinaesthesis experienced when these movements, and these movements only, are made.

The advantages of such an objectively 'checked' language are clear. A name given to a kinaesthetic experience would relate to the 'inner' feeling of some movement, set of movements or attitude the nature of which had been objectively demonstrated so that its meaning would be constant for everyone. Such a movement could be carried out again and again until its 'feel' became unmistakable. Related movements might be given related names. Movements common to several occupations might be analysed out and their relations to these different functions studied, as Mr Gilbreth has suggested. In the hands of skilled and educated teachers, the supplementary teaching of movement by pictures, diagrams, models, and the slowed-up cinematograph might then become really effective. Visual 'representations' of movement, obtained by the use of the apparatus of motion-study, would really represent and not caricature the motions which they profess to depict. This would inevitably lead to the detection of the less desirable methods of carrying out any movement, and therefore, again, to increased skill, or kinaesthetic knowledge. Not only would the status of such knowledge be raised, but also the status of those people who, through their teaching, aid in its acquisition and development.

VII. IMPROVEMENT OF THE SOCIAL AND INTELLECTUAL STATUS OF KINAESTHETIC KNOWLEDGE.

In a psychological discussion of this subject it is obviously relevant to comment upon the present disparagement, by an influential section of the community, of kinaesthetic knowledge. The tradition that knowledge worth having is almost exclusively confined to that which has reached us through our eyes and ears has been confirmed and hardened by the powerful mechanisms of class distinction and class tradition, and by generations of a certain type of teacher in school and University. To such influences the ordinary man owes his view of culture. From most people, therefore, the motiles, as compared with their socially-established brothers, the visiles and audiles, seldom get fair play. The two latter groups, themselves recognised as belonging to a kind of *intelligentsia*, not infrequently fall into the bad habit of regarding with some contempt all persons whose motor activity is expressed through channels other than the socially-approved ones of speech and writing[1]. A vicious circle is thus made; many intelligent persons are never encouraged to contemplate the study of such non-verbal occupations, and not a few are actively prevented from taking them up. This is one reason why the intelligent professional in sport, the intelligent teacher of handwork, and the intelligent teacher of games are still so much of a rarity that when they do appear one never fails to remember them. If the knowledge obtainable through kinaesthesis were increased and transmitted to others in the best and quickest ways by teachers who were themselves good performers, their social status would rise. But this is the less important consideration; their intellectual status would be improved too. By increasing their kinaesthetic experience, they would not only have gained deeper knowledge of a comparatively new aspect of their world, but such knowledge might form basic material for the elaboration into concepts which characterises intellect in the second sense of the term; the process by which we acquire the knowledge of truths as distinguished from the knowledge of facts.

I have attempted elsewhere[2] a crude analysis of the process of thinking. Its essence seems to be the recall of past experiences, abstraction of their relevant aspects, their comparison, re-comparison with some aim in view, combination of the results of this comparison into a 'new' conclusion, and the expression of this conclusion in action, gesture, speech or writing. All this happens so regularly when its raw material is formed by visual or auditory experiences that many people are apt to forget that this series of processes may be carried out just as successfully upon the basis of touch and kinaesthetic memory[3]. Few would have the hardihood to object that in such a case the processes were not intellectual. If they did, Helen Keller, with her university degree and her literary achievements, would provide the answer.

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[1] Mr Bertrand Russell has placed on record his suspicion that what people should mean by intellect is simply "certain habits in the use of words," and his lack of "mystical reverence for these habits" (*Mind*, xxix, N.S. No. 116).

[2] This *Journal*, 1920, xi, 71–4.

[3] May it be that really superlative excellence in games or manipulative labour is distinguished from mere 'goodness' by a much greater degree of development of the process of working up the raw kinaesthetic material into new combinations; a particularly good example of 'kinaesthetic intellect'?

VIII. SOME FURTHER SPECULATIONS.

The relation of these considerations to the doctrine of *Bewusstseinslagen* or "conscious attitudes" is obvious and interesting. It seems certain that the thinking which goes on in the possessors of a rich and pliant kinaesthetic memory tends to be strongly influenced by such conscious attitudes, perhaps to the discouragement of visual and auditory images. How far the left wing of behaviourism is constituted by psychologists with this type of mentality, whether, consciously or unconsciously, they contemn the visual-auditory intellect in much the same way that many intellectuals contemn the kinaesthetic variety; to what extent and in what ways these different kinds of mental apparatus may assist in the formation of those different attitudes towards life which are called introvert and extrovert respectively; the rôles which different kinds of imagery play in aesthetic appreciation, in the causation and the cure of mental disorders,—all these problems are related to this subject.

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A NEW METHOD OF MARKING GROUP TESTS.

By CHARLES FOX.

THE distinguishing feature of Mental Tests such as Binet's is constituted by the fact that the tests are graduated by being tried on normal unselected persons. Such tests have been widely used to discover the mental age of school children, but are not aimed at discovering the existence of any special ability. As their purpose is primarily to assign a 'mental age' they are too crude to be used for discriminating grades of excellence within any mental age, and any attempt to use them in this way is due to a misconception of their origin. The main object of this paper is to suggest a new method of detecting differences within any group of persons.

It has recently become the custom to hold an annual examination of the children in elementary schools in various counties and boroughs in order to pick out those who are fitted to receive a higher education. The examinations usually consist of the ordinary school subjects such as English, Composition and Arithmetic and attempts have been made to devise appropriate 'tests' to take the place of, or to supplement such examinations. In an ordinary borough the numbers of children dealt with are too large to enable tests to be given individually by the examiners. It must be admitted at once that any 'mental test' which is not given individually loses a good deal of its discriminative value.

The Cambridge Borough consented this year to include a mental test as part of its examination in order to try the value of this procedure, but the candidates for higher education were selected in the ordinary way by examination. All the children in the elementary schools of the Borough, without exception, over the age of eleven and under twelve on September 1st, sat for the examination in the preceding June; the subjects being Arithmetic, Reading, Composition and Dictation. There were 336 boys and 303 girls of this age. At the same time a 'mental test' was given to the candidates. Only 15 minutes could be allowed for this test and it was necessary therefore to devise some Group Test which appeared likely to yield satisfactory results in such a limited time. It was decided to make use of Burt's reasoning tests[1] but to mark them on a new plan. The tests were selected from those which Burt assigns to the ages 7 to 13 years using only the more thoroughly revised ones and making certain minor verbal alterations which experience had shown to be necessary. A preliminary trial on selected children of the age range 11 to 12 yielded an estimate of the amount that could be attempted by brighter children in the time allowed.

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[1] *J. of Exp. Ped.* June and Dec. 1919.

The ten following questions were chosen:

1. I have bought the following Christmas presents: a pipe, a blouse, some music, a box of cigarettes, a bracelet, a toy engine, a bat, a book, a doll, a walking stick, and an umbrella. My brother is eighteen; he does not smoke, nor play cricket, nor play any instrument such as the piano. I want to give the walking-stick to my father and the umbrella to my mother.

Which of the above shall I give to my brother?

2. I don't like sea voyages, and I don't like the seaside. I must spend Easter either in France, or among the Scottish hills, or on the South coast.

Which shall it be?

3. Three boys are sitting in a row. Harry is to the left of Willie; George is to the left of Harry.

Which boy is in the middle?

4. Violet has caught an illness. If she has a rash it is chicken-pox, or measles, or scarlet fever. If she has been ailing with a cold or cough she will develop whooping-cough, or measles, or mumps. She has been sneezing and coughing for some days, and now spots are appearing on her face and arms.

What do you think is the matter with Violet?

5. Father has just come home in a brand new overcoat; there is clay on his boots and flour on his hat. The only places he can have been to are Northgate, Southgate, Westgate or the City. He has not had time to go to more than one of these. There is no clay anywhere in the streets except where the pavement is up for repair. There are tailor shops only in Southgate, Westgate, and the City. There are flour mills only in Northgate, Westgate, and the City. I know the roads are not being repaired in the City, though they may be in the other places.

Where has father been?

6. Where the climate is hot, gum-trees and rubber will grow; heather and grass will only grow where it is cold. Heather and rubber require plenty of moisture; grass and gum-trees will grow only in fairly dry regions. Near the River Amazon it is very hot and very damp.

Which of the above grows there?

7. In a school a third of the children play football and a third play cricket.

(1) Are there any who play neither football nor cricket?

(2) Can you tell without any more information whether there are any who play both?

8. Field mice devour the honey stored by the humble-bees; and honey, which they store, is the chief food of the humble-bees. Near towns there are far more cats than in the open country. Cats kill all kinds of mice.

Where, then, do you think there are most humble-bees—near towns or in the open country?

9. I started from the church and walked 100 yards; I turned to the right and walked 50 yards; I turned to the right again and walked 100 yards.

How far am I from the church?

10. A pound of meat should roast for half-an-hour; two pounds of meat should roast for three-quarters of an hour; three pounds of meat should roast for one hour; eight pounds of meat should roast for two hours and a quarter; nine pounds of meat should roast for two hours and a half.

From this can you discover a simple rule by which you can tell from the weight of a joint how long it should roast?

As is well known a 'mental test' is designed to set all persons on a level with regard to acquired knowledge, and data are consequently selected which involve no special attainments but simply those which a normal individual is bound to acquire in his every-day dealings with his physical and social environment.

One thing, however, is frequently overlooked. A game can only be properly played when the players agree explicitly or tacitly to abide by the rules of the game. This agreement gives a certain 'set' of mind. In an examination this 'set' is assumed by the examiners; and in giving a mental test the experimenter takes for granted that the person will assume this attitude of 'make believe.' Now a bright child may resent this attitude and refuse to be bound by it. An incorrect answer may therefore only mean that the child has adopted some 'set' of mind which the examiner did not intend. In question 10 for instance the child who says that the length of time a joint should roast depends on "the kind of joint" is refusing either deliberately or unconsciously to adopt the attitude of mind towards the question which the examiner assumes. Again, in question 1 a child who wishes to give his brother a cricket bat though he does not play cricket may desire that his brother should learn cricket; or if he gives him a box of cigarettes though he does not smoke, he may think that his brother may wish to give the cigarettes to a friend. A candidate, again, who in question 7 says that the boys play both cricket and football and the girls neither may be thinking of the suitability of the games for the sexes. All the above replies were frequently given. The attitude of 'supposal' with reference to a given 'universe of discourse' which the question delimits is assumed to be the attitude which the candidate will take spontaneously. But this assumption is not universally valid. It sometimes happens with bright children that they regard the questions as ridiculous and the procedure as silly and so refuse to play this particular game. A group test cannot take this incalculable factor into account, except by assuming that the ability to make definite suppositions is an essential part of intelligence. With these considerations in mind the children were instructed to answer the questions in order as printed. They were told that each question had one definite answer and that there was no 'catch' in any of the questions.

It should be remembered that the object of this test was not to discover the mental age, but to pick out the brighter children of the whole group. A difficulty arises from the fact that some children work faster than others. If their work is accurate they should obviously get credit for this speed but the marking should be designed mainly to discriminate ability within the Group.

In essence Binet's principle, which has been used by all subsequent investigators, is to use a standard group to mark all other groups or individuals. The attempt is made in this article to modify the principle so that each group may assign marks to the individuals within the group. The tests should be marked for each individual in proportion to the difficulty which the group as a whole finds in dealing with the test. The children were working under the same external conditions and with the same preliminary instructions. Assuming that they also accepted the condition of 'supposal' suggested by the tests the following scheme of marking gives credit chiefly for ability to cope with the test. All the questions yield an unambiguous answer.

After the papers had been marked a table was drawn up of the number of boys and girls separately who had answered each question correctly. The table was as follows:

		1	2	3	4	5	6	7	8	9	10
No. of Candidates answering correctly	Boys	166	215	178	121	76	71	140	95	48	17
	Girls	157	181	170	154	77	90	69	67	28	9
Percentages	Boys	49 %	64	53	36	23	21	42	28	14	5
	Girls	52 %	60	56	51	25	23	23	22	9	3

The reciprocals of some function of these numbers yields a measure of the difficulty experienced by each group in answering the separate questions. A glance at the table will show that the reciprocals of the numbers themselves would give undue weight to those who were able to work more rapidly. It is therefore necessary to choose such a function as will ensure the same serial order whilst placing more stress on accuracy as compared with speed. Raising each number to some fractional index would accomplish this but the most convenient mathematical function appears to be the logarithms of the separate numbers. A consideration of the logarithmic curve shows that the further we proceed the more relatively difficult does scoring become. A candidate finds it more and more difficult to score marks in the later questions and he cannot sacrifice accuracy to speed without being penalised. It is better to work accurately than speedily but accurate rapidity yields higher marks. Hence the logarithms of the above numbers were found and the reciprocals of the logarithms were used in marking the questions. This method of weighting provides a new and convenient way of discriminating differences such as those due to sex in dealing with mental tests.

The following numbers are closely proportional to the reciprocals of the logarithms:

Question	1	2	3	4	5	6	7	8	9	10	Total
Boys	5	5	5	5	6	6	5	6	7	9	59
Girls	5	5	5	5	6	6	6	6	8	12	64

The papers were now examined a second time and the marks found in the second table were assigned to the correct answers. By this procedure each boy and girl was measured by the members of his own group and received marks for each question which varied with the difficulty found by the whole group in dealing with the question. The actual performances of the boys and girls therefore determined the marks given for each test.

It is seen that with regard to difficulty the questions fall into three groups thus 1 to 4; 5 to 8; 9 to 10; and that boys and girls responded to the first nine questions in the same degree but the girls found question 10 much more difficult than did the boys. Questions 7, 9 and 10 are questions involving mainly mathematical concepts and in these the boys scored better than the girls. Question 4 was answered correctly by relatively a greater number of girls than boys and the reason would appear to be that they were more interested in the subject matter; as their replies even when wrong showed an observation of symptoms of illness not displayed by the boys. We are justified in the belief that the ability to cope with a mental test depends on the mental 'set' or attitude towards the subject matter of the test.

The examination in school subjects was conducted by a committee of head teachers and the Local Education Authority, and was carefully marked on an agreed plan. In Dictation the boys were penalised more than the girls for mistakes, which explains the great discrepancy in the marks. The Arithmetic test consisted of 12 questions on simple rules and very simple problems to be done in 1¼ hours; the Composition consisted of an essay to be written in 1¼ hours and was marked for style only; and a headmaster and headmistress respectively heard each individual boy and girl read. Although the marks of the two sexes are not strictly comparable we may compare the standard deviations in each subject.

It will be seen from the table that the boys on the whole are more variable than the girls in the examination subjects, with the exception of reading where the difference is slight.

The Mental Tests were all marked by the writer of this article assisted by his pupils, and as each question yields an unambiguous answer the marks are strictly comparable. Very little difference was found between the sexes; both the average mark and the standard deviation are practically the same. This result agrees with the work of Terman[1] who found that there was practically no difference in the Intelligence Quotients of boys and girls between the ages of 5 to 14 years.

Average marks in the Examination and Tests.

	All School Subjects (Max. 300)	Std. Dev.	Arithmetic (Max 100)	Std. Dev.	Composition (Max. 100)	Std. Dev.	Reading (Max. 50)	Std. Dev.	Dictation (Max. 50)	Std. Dev.	Mental Tests		Std Dev.
											Boys (59)	Girls (64)	
Boys	113·3	65·5	33·2	26·4	38·0	23·0	28·6	12·3	13·8	15·0	16·9	(28·6 %)	11·3
Girls	156·9	58·0	30·2	24·9	53·7	18·3	37·0	11·5	36·4	12·7	17·7	(27·6 %)	10·5

The candidates were selected for scholarships entirely on the examination results, no regard being paid to the results of the mental tests except in about half a dozen border line cases. All the candidates who gained more than half marks in the examination were chosen for scholarships, namely 282. We are able therefore to compare the results of the examination with the independent results of the mental tests. By selecting all the children who gained over 20 marks in the tests it was found that only 48 of these were not chosen for scholārships; in other words good scoring in the tests made it highly probable that the candidate would be chosen by the examination[2].

The Frequency-distribution of Marks in the Mental Tests.

Boys

Marks (max. 59)	0–6	7–13	14–20	21–27	28–34	35–41	42–48	49–55	Total
Candidates ...	68	73	93	44	34	10	10	4	336

Girls

Marks (max. 64)	0–6	7–13	14–20	21–27	28–34	35–41	42–48	49–55	Total
Candidates ...	47	68	79	58	28	17	5	1	303

In order to get a statistical comparison the coefficients of correlation were calculated. The coefficients show that there is a high correlation between the performance in the whole examination and the results obtained by the reasoning tests. As might be expected, the correlations are highest between the tests and the Arithmetic papers especially in the case of the boys. With regard to the girls, the coefficients of correlation diminish as we pass from Arithmetic to Composition, thence to Reading and Dictation. With the boys, however, the correlations between Composition, Reading and Dictation respectively and the reasoning tests are practically the same; being in each case much lower than the correlation with Arithmetic. The following table shows that the results are trustworthy as the probable errors are very low in every case.

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[1] *Measurement of Intelligence*, Harrap, 1919, p. 68 ff.

[2] An instructive case occurred. A boy whose total marks were only 101 was chosen by the Headmaster who heard the reading on the ground of General Intelligence. It was subsequently discovered that his marks in the Tests were very high, namely 30.

Table of Correlations.

		All 4 School Subjects	Arithmetic	Composition	Reading	Dictation
Mental Tests	Boys ...	+·62 ±·023	+·64 ±·022	+·47 ±·028	+·48 ±·028	+·46 ±·029
	Girls ...	+·57 ±·026	+·56 ±·026	+·47 ±·030	+·43 ±·030	+·38 ±·030

Burt[1] maintains that performances in school subjects depend upon 'General Educational Ability' which is a factor common to all elementary school subjects and also upon group factors of Specific Educational Abilities. The above results appear to confirm his views as regards the General Factor and to support the existence of the group factors but they tend to show that Composition should be included in Burt's linguistic group instead of being separated from Reading and Dictation as he suggests.

The writer desires to thank Mr G. Udny Yule for his generous advice in dealing with the statistical results.

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[1] *Distribution and Relations of Educational Abilities*, London, P. S. King, 1917.

(*Manuscript received* 2 *August* 1921.)

CRITICAL NOTICE.

By CHARLES FOX.

Education and World Citizenship: an Essay towards a Science of Education. By JAMES CLERK MAXWELL GARNETT. Cambridge: University Press, 1921. pp. x + 515. 36*s*. net.

This is the most ambitious attempt made in recent times to construct a science of Education on the basis of scientific principles. In building the edifice the author uses the results of the most recent relevant research in experimental psychology and elaborates an epistemology of modern science. The final section of the book is a survey of the types of education and educational institutions as they might exist in England if we desired to have a complete and consistent national system of Education.

In Book I the author follows closely Professor Adams's account of the origin and growth of the doctrine of 'formal training' and rejects this doctrine completely. This theory which is better described as the doctrine of the 'transference of training' rests on the assumption that the results of education in any sphere of activity are transferable to other spheres; or as a schoolmaster once said, "It does not matter a bit what you teach a boy provided he thoroughly detests it." Apparently this master believed that the will is strengthened by constant exercise in dealing with material which the pupil would avoid if left to himself. It is curious that the author first rejects this view and in Book II quotes with approval Dr Temple's dictum that "It scarcely matters what subject is taught: the vital matter is that the child should learn 'attention' in general."

The main part of the second book is concerned with an elaborate attempt to show that the materialistic view of the structure of mind may be safely used as a working hypothesis in dealing with the problem of education. With this object physiological and psychological data are used interchangeably so as to establish the doctrine that there is a permanent record in the brain of all life's experience. Instead of the better known term 'engram' the word 'neurogram' suggested by Morton Prince is adopted for such records. Any system of connected neurograms of which one at least is a cognitive neurogram is called an interest-system, and by using McDougall's hypothesis of drainage the principle is laid down that "of all active neurograms that one which is connected with the widest and deepest interest-system tends to drain the impulses from all of them." In order to carry the argument further some useful technical terms are employed. An interest-system and especially a "future-interest-system" is assumed to be constructed in such a way that the more central elements within it exercise the greatest influence on the flow of nervous excitement. Such a system is called an 'endarchy' as it is ruled from within outwards. An endarchy which is so organised as to yield the minimum average path between any one element in the neurography and any other, and therefore gives rise to the shortest trains of reasoning within the system, is called a 'maximal endarchy.' If all sciences were complete, containing no redundant facts or essences; and if all the reasoning within the whole body of such sciences were 'elegant' in the mathematical sense of the word; and if all were united into one philosophical system, we should have the maximal endarchy of science. An omniscient being who was familiar with this system and had been trained to reason perfectly with the minimum expenditure of mental effort would have neurograms corresponding to the essences of the endarchy of science! The neurograms of such an endarchy would constitute a sort of magnified tree of Porphyry with the branches not limited to two at each bifurcation. Most people would admit with Poincaré that there is a hierarchical arrangement of facts within a particular body of science, the order of importance of which would be recognised by experts; but it is extremely doubtful whether we ought to go further with the author and assume that at some distant future time this order will assume a final form corresponding to the maximal endarchy of science. Just as that myth of Spencer's, the completely adapted man in the completely evolved society, has not aided the science of Ethics or pointed out the path to correct conduct, so the maximal endarchy of science seems equally unfitted to give guidance in education, and for the same reason. Both conceptions ultimately assume that the path of progress leads to a stage when progress will cease. No doubt in considering educational procedure it is wiser to try to coordinate the pupil's knowledge into an organic whole rather than to leave the various parts disjointed, and an ideal of complete organisation aids this process. But there are branches of education in which the procedure to be adopted is totally different from that used for cultivating organised scientific knowledge, namely those which deal with aesthetic and religious education. The attempt to teach such a subject as literature as part of the organised system of scientific knowledge resulted in those text books which contained a few pages of text and a volume of notes which the unfortunate pupil was expected to learn, and which prevented him ever afterwards from appreciating the text.

In dealing with the question of 'purpose' the inadequacy of the neurographic scheme from the standpoint of education is apparent. The crux of the discussion turns on the view that "everyone of my present purposes whatever the date of its fulfilment is represented in my brain by a neurogram." For this reason Dr Webb's Group Factor in character qualities (*w*) which is intimately related to purpose is taken to correspond with certain neurograms. This can be done only if we are willing to admit that the sole meaning which can be assigned to the idea of 'importance' is that of 'depth' of neurograms. Surely, however, the importance of an event, a fact or essence is quite distinct from the number of times it is thought about by an individual. Habitual routine usually occupies most of a man's waking life and the neurograms corresponding to it are presumably deeper than anything else which he has acquired, but that does not make routine more important than the single original idea which may occur to him once in a lifetime, and which may have a supreme importance belonging to a totally different category.

In treating of neurographies a very valuable suggestion is made, namely, that the growth of scientific knowledge enables us to organise our thoughts so as to avoid mental conflict. The reason that knowledge is organised similarly in each individual is due, apart from the influence of the social environment, to the operation of the same instinct, namely, curiosity. The author also suggests that the aesthetic pleasure yielded by the operation of organising our knowledge so as to form an endarchy is due to the satisfaction of the emotion which is united with the instinct. Thus it follows that ultimately the ground of the maximal endarchy of science is our instinctive endowment. Left to itself the instinct of curiosity would result in numerous intellectual conflicts but by the operation of Reason a solution is brought about. If our knowledge is to be true our neurography "must correspond, element for element, with the endarchy of science." Since however the structure of the neurographies has been invented to fit in with the facts of knowledge and to make the psychology of the book complete, there would not seem to be any meaning in their failing to correspond. The neurographies having been constructed to fit the psychological facts, it is not at all to be wondered at that a correspondence is found, since it has been put there from the start. This may be admitted without detracting from the value of the advice that the education of any pupil in school should be in the hands of the smallest possible number of teachers and as large a proportion of his time as possible should be with form masters as opposed to specialist teachers; for the form masters are to lay the basis of the single wide-interest-system of the pupil.

The most valuable chapter in Book II is that dealing with Will and General Ability which treats a difficult subject lucidly. A useful distinction is made between 'Will' at short range *i.e.* a single effort of will, and 'will' at long range which consists in the persistence of motives. The latter is called 'purpose' and it would clear several psychological discussions if this distinction were uniformly adhered to. The discussion of the relations between the General Factor (*g*) and the group factors of Dr Webb and the author is thorough and suggestive, ably summing up the work on the subject. From the educational point of view it is important to learn that whilst the 'Will' is educable the degree of its educability is probably innate.

The argument of the several chapters on conduct, character and the aim of education rests on the tacit assumption noted above that the concepts of 'importance,' 'value,' 'worth' etc. may be equated with the concept of 'depth.' The purpose neurograms are supposed to be more important because they are 'deeper' and "the influence of Purpose upon conduct is wholly accounted for by the corresponding purpose-neurograms." So "A man's thought is determined by his neurography and his Will." Consequently "his character is determined when his neurography and his Will are determinately known." In the future when (*g*) is measured we shall only have to investigate a man's nervous system to know him all in all! The conversion of a personal endarchy into a religion is according to the author straightforward. The neurography determines his philosophy, and united with emotional elements it is transformed into a religion. If, however, men are to live together in society there must be a harmony of their central purposes and this can only happen if each has a single maximal endarchy corresponding to the endarchy of science. The central elements in the endarchy will ensure this because they are deepest and therefore most frequently come into activity. Harmony, too, is therefore due simply to depth of neurograms.

But the most important and fruitful facts in science are not yet discovered and it would seem therefore that maximal endarchies are not attainable. The author, however, has a solution which makes it possible for him to harmonise all neurographies in a maximally progressive society and to state the aim of education for all world citizens. No society can be maximally progressive which is liable to interference from rival societies. Hence such a society must include the whole human race, and a League of Nations is necessary in order to formulate the ultimate aim of education. Before the League can get to work properly, it is essential to get a clearer view of the undiscovered central portions of the endarchy of science. Bacon long ago suggested that the world of knowledge might be mapped out and a college of research students set to work. The author however is satisfied that the Christian account of the universe adequately "completes the discovered part of the endarchy of science with a hypothesis concerning the hitherto undiscovered central essences." When science is so completed we arrive at the Kingdom of God as interpreted by Christianity. Thus the aim of Education, the world over, is to form Christian characters. It may be difficult to persuade the Japanese, Chinese and Hindu members of the League to adopt this hypothesis, but until it is done world citizenship is obstructed and the maximal endarchy of science impossible.

Book III deals with a national system of education which might be established in England in the next decade and is the most comprehensive account of types of education and of schools and colleges in the English language. It is conceived and worked out on large plan reminding one of the plan of Comenius except that it is more practical and based on existing institutions. The author's suggestions are illustrated by a 'flow diagram' drawn to scale indicating types of education by colours and varieties of schools and colleges by lines. The diagram will repay careful study, not only by students of education but by administrators, as it is eminently practical and can be adapted to different areas. All this Book is extremely important but is outside the purview of this Journal. There is one section however which should be of interest to all readers, namely a distinction between two types of education, or two distinguishable ways in which the same subject may be studied. For those who devote many years to any subject the study should be 'transitional' aiming at coherence rather than comprehensiveness, dealing with abstract conceptions and root ideas, capable therefore of growing after formal education is finished. Those whose period of education is more limited should learn in a 'terminal' way *i.e.* mainly through concrete facts and wider concepts, treating the subject from the outset comprehensively and aiming at width rather than depth. The difference may be expressed by calling the latter multifocal and the former unifocal. In other words boys who leave school at the age of 16 to enter occupations should have different courses, though they deal with the same subjects, from those studied by boys who intend to stay at school till 18 or 19 and then proceed to the University.

It remains to add that there are several useful appendices to elucidate the text.

PUBLICATIONS RECENTLY RECEIVED

An Introduction to the Psychological Problems of Industry. By FRANK WATTS. London: George Allen & Unwin Ltd. 1921. pp. 240. 12*s.* 6*d.* net.

The Psychology of Industry. By Dr JAMES DREVER. London: Methuen & Co. 1921. pp. xi + 148. 5*s.* net.

No doubt a case could be made out for the publication of both of these books. They appear in response to a popular demand. They are fairly well-informed, and although both are hastily written, are undoubtedly of considerable general interest. Dr Drever's is perhaps the better of the two, for he confines his discussions to matters about which at least some things are definitely known. More than half of Mr Watts's book deals with problems in social psychology which are hardly worth discussing at all unless they can be considered very thoroughly. "The springs of conduct" cannot be made clear in six and a half pages, nor can the psychological characterization of State socialism and syndicalism be adequately dealt with if only a couple of pages are devoted to each. Mr Watts in fact hunts in too many directions to bring down any very valuable quarry. In the case of both of these books there is exceedingly little evidence that original research has been attempted. The old illustrations, that seem to have appeared already in all the books that have ever been written on industrial psychology, come up again. Again we learn about the pig iron, the bicycle balls, the trench digging, the brick laying, and the American Army tests. It is not that these illustrations are nòt striking, or useful, or well worthy of notice; but only that by repetition they become wearisome, and that they are not enough to base a whole science upon.

The Science of Ourselves. By Sir BAMPFYLDE FULLER, K.C.S.I. London: Henry Frowde and Hodder & Stoughton, 1921. pp. x + 326. 16*s.* net.

This is an interesting book, on account of a certain originality of out-of-date thought, not because of its psychological value, as the following quotation serves to illustrate—"All pleasures and displeasures involve the intervention of the brain, since it is this which renders them *conscious*, by combining motor excitement with sensory excitement. When the motor excitement is simply the effect of sensory impressions upon the brain...it gives pleasure of the subdued type which we describe as 'interesting.' It becomes aggressively pleasurable or displeasurable when it is sympathetically controlled or intensified by the vibrations of motor excitement on the psychic plane" (226).

The Beloved Ego: Foundations of the new Study of the Psyche. By Dr WILHELM STEKEL. Translated by Rosalie Gabler. London: Kegan Paul, 1921. pp. xv + 237. 6*s.* 6*d.* net.

The nineteen chapters of this book, dealing with doubt, the fear of joy, envy, impatience, etc., are delightful reading. The writer is a poet as well as a physician. "It is a generally accepted fact," he declares, "that a doctor must be an artist." He claims to have retained the best of Freud's teaching and to have evolved from it a new one.

How to Use your Mind. By Dr H. D. KITSON. Philadelphia: J. B. Lippincott & Co., 1916. pp. 217.

The author was Instructor of Psychology in the University of Chicago, where special attention is given to "supervision of study. All freshmen in the School of Commerce and Administration of the University are given a course in Methods of Study, in which practical discussions and demonstrations are given regarding the ways of studying the freshman subjects" (4). The book deals in a popular, common-sense way with note-taking, first aids to memory, examinations and kindred topics.

Anatomy of the Nervous System: from the Standpoint of Development and Function. By Professor S. W. RANSON. Philadelphia and London: W. B. Saunders & Co., 1920. pp. 395. 32*s*. 6*d*.

The title of this book indicates the author's point of view. He is to be congratulated on the result of his labours. The book contains 260 illustrations, some in colour, all beautifully reproduced, and most of them made from drawings specially prepared for the author. Sherrington's classification and terminology have been adopted.

How to Measure. By Professors G. M. WILSON and K. J. HOKE. New York: The Macmillan Co., 1920. pp. vii + 285.

This book is intended to serve as a text-book in the training of teachers. It does not attempt to estimate the value of all available tests, but describes only those "which on account of their use, purpose and adaptability have been found to be most serviceable." Successive chapters deal with the measurement of spelling, handwriting, arithmetic, reading, composition, drawing, general intelligence, etc. References solely to American literature are added to each chapter.

Measure your Mind. By F. P. STOCKBRIDGE and Dr M. R. TRABUE. London: G. G. HARRAP & Co., 1920. pp. v + 349. 10*s*. 6*d*. net.

This is a well-written, easily readable book, giving a very useful account of a large number of tests. These, however, are of somewhat unequal value, and hence are not to be adopted without discrimination.

Trade Tests: the Scientific Measurement of Trade Efficiency. By Dr J. C. Chapman. London: G. G. Harrap & Co., N.D. pp. vii + 435. 15*s*. net.

The contents of this book are in great part pamphlets which have been already published, describing the trade tests devised to meet the needs of the American War Office. It is disappointingly lacking in constructive criticism.

Dr Klages's Entwurf einer Charakterkunde. Von Dr O. HERMANN. Leipzig: J. A. Barth, 1920. S. iv + 63. Mk. 15.

This is a précis of Klages's *Prinzipien der Charakterologie*, issued in the same year by the same publishers, according to which the 'material' of character is classifiable into ideational capacities varying in quantity, quality, distinctness and mobility, and into apperceptive dispositions varying in grade, direction and form: while the 'structure' of character is constituted by a number of factors according as it is of the affective or volitional type. A detailed account of this condensed presentation by the author of an undoubtedly difficult book is, however, quite impossible here.

Psycho-analysis, Sleep and Dreams. By ANDRÉ TRIDON. London: Kegan Paul. pp. xii + 160. 7*s*. 6*d*. net.

This is a popular, 'scrappy' book about sleep and dreams. Its journalistic style may make it attractive for some readers, but it cannot be regarded as having any particular interest for the serious student.

NOTES ON RECENT PERIODICALS

Journal of Experimental Psychology, Vol. III

No. 5, October 1920.

The Effect of Fatigue on Retention (John J. B. Morgan).

When a period of learning is long continued, a large amount of material learned, and the learning partial only, the capacity of retention may diminish considerably in the later stages of the task. Under the same conditions, the recognition of material used in the later part of the series presented is defective. This is interpreted to mean that adaptation to fatigue involves a narrowing of attention to the precise task on hand.

Effects of Smoking on Mental and Motor Efficiency (Sven Froeberg).

The experiments support the view that there is no reliable evidence that the moderate use of tobacco produces either beneficial or harmful effects in a measurable degree.

A Study of Ocular Functions with Special Reference to the Look-out and Signal Service of the Navy (C. E. Ferree, G. Rand and D. Buckley).

The conditions under which satisfactory tests of keen visual discrimination in low illumination must be carried out are discussed. The most useful type of test must determine directly a candidate's power to see clearly at low illumination.

Block-Design Tests (S. C. Kohs).

Performance tests, in which blocks painted in different colours have to be combined so as to produce certain designs, are described, and some of their results discussed.

Recent Apparatus from the Psychological Laboratory of McLean Hospital (F. L. Wells and C. M. Kelley).

The apparatus described comprises: a galvanometer control board, a "fall" apparatus, a choice reaction switchboard, a "fall" screen, a multiple pen chronograph, and a water recording device useful in relation to tests of motor steadiness or tremor.

Sex Differences in the Effect of Discussion (Harold E. Burtt).

Experiments on the effect of discussion upon decisions show that there is "considerable tendency to change one's decision as a result of discussion, but the change is as often in the wrong direction as the right." No appreciable sex differences are found.

Practice Effects in Intelligence Tests (Knight Dunlap and Agnes Snyder).

It is shown that the practice effects in the taking of intelligence tests may be considerable. A plan for equalising these, as far as possible, for different groups of reaction is proposed.

No. 6, December 1920.

Psychological Tests used for Selecting Aviators (G. M. Stratton, Henry C. McComas, John E. Coover, English Bagby).

The tests which seem best adapted to determine aviational ability are: (1) judgment of relative speeds of moving objects, (2) judgment of parabolic curves, (3) complex reaction time. These must be given further trial.

The Relative Value of Grouped and Interspersed Recitations (E. B. Skaggs).

Taking it as proved that recitation (attempted recall) facilitates the learning process more than mere reading, the experiments show that to alternate readings and recitations produces better results than to group both. The results are more conclusive for nonsense than for sense material.

Results of some Experiments on Affection, Distribution of Association and Recall (C. H. Griffiths).

The most interesting conclusions are: (*a*) pleasant associations tend to be more numerous than unpleasant associations, (*b*) affective reaction of any kind is favourable to recall, there being a slight preference for pleasant associations, (*c*) there is evidence for the existence of a general affective sensitivity.

Two New Time Control Instruments (Donald A. Laird).

(i) "An acceleration time control apparatus," consisting essentially of an inclined U-shaped groove, with alternating strips of insulating and conducting material, and (ii) a disc-commutator time control instrument are described.

Journal de Psychologie normale et pathologique (28^{me} Année)

No. 1, January 1921.

Sur le psychisme inconscient (Fr. Paulhan).

All psychologically distinguishable processes—such as perception and recognition—may be carried on unwittingly. If afterwards they should become known, their nature, as facts of mental structure, is in no way affected, though they may acquire new associations. What has been unwittingly apprehended may later be consciously recognised.

Le rire (G. Dumas).

Five problems are distinguished and discussed: (1) what are the physiological and anatomical mechanisms of laughter? (2) what are the main conditions of laughter considered as an expression of well-being? (3) why is it that the perception of certain relations are considered comic? (4) by what psychophysical mechanism does laughter result simply from the perception of these relations? (5) in what ways may laughter be said to constitute a social language?

La conscience et la conscience du moi (H. Wallon).

The development of the consciousness of self as contrasted with consciousness of a world of extended objects is discussed. Eventually the self is conceived as wholly independent of all spatial determinations.

No. 2, February 1921.

De l'automatisme dans l'imitation (H. Delacroix).

Various forms of imitation and their accompanying mechanism are analysed. Their development and application are studied particularly in reference to language. The precise relation between perception and movement in imitation is given particular consideration.

Les oscillations de l'activité mentale (P. Janet).

A course of study in the conditions determining the degree of intensity of different psychical responses is sketched. Special reference is made to variations in emotional expression in the normal and the abnormal subject.

Sur le psychisme inconscient (Fr. Paulhan).

The unconscious is considered in reference to tendencies and desires, and it is found that these, just as much as cognitive and affective processes may proceed unwittingly. In fact the unconscious mental life may be described in just the same terms as the conscious mental life. Underlying both are the same tendencies, the same structure, and both have to deal with very much the same material.

André Gilles writes a short note on the illusion of false recognition, and a paper by Etienne Rabaud on a "Contribution to the Study of the Instinctive Behaviour of Certain Spiders," read at a meeting of the *Société de Psychologie* is reproduced.

No. 3, March 1921.

Théorie de la perception (B. Bourdon).

The paper discusses both the general psychological character of perception, and certain special problems, including: the perception of space, whether visual or auditory, the perception of movement, and the development of spatial perception. Most of the traditional questions are discussed.

L'orientation lointaine (E. Claparède).

The ability to pursue a goal which cannot be immediately perceived by the individual who seeks it, is here studied in detail. A number of proposed explanations are discussed. The problem is itself analysed. The general conclusion is that many complex problems are involved, which are as yet far from being satisfactorily settled.

La folie, pensée organique (A. Hernard).

The organic basis of various forms of nervous disease is studied in its relation to the conscious and symbolic expression of such disease.

Caractère individuel et aliénation mentale (W. Boven).

The purpose of this paper is to show precisely how investigations into individual character may throw light upon the real causes of mental disorder. The study of individual differences should take into account, not only the mental development of the patient himself from his earliest years, but also the characters of his immediate ancestors.

No. 4, April 1921.

Sur le centre de la faim (J. P. Pawlow).

There is a thoroughgoing analogy between the respiratory and alimentary centres. The latter possesses complex functions which demand analysis. Its localisation is uncertain, but it undoubtedly must be conceived as distributed to different positions within the central nervous system. It is a regulating nervous structure determining the reception of solid and liquid material necessary for vital chemical changes.

Le mécanism de la pensée (G. Revault d'Allonnes).

The paper is presented as a criticism of Taine. Its main contention is that the notion of 'image,' widely used in the psychology of thinking, is confused and misleading. For images, schemata must be substituted, when the difficulties incident to the image-type of psychology can be overcome.

Activité mentale, travail intellectuel et fatigue (G. Poyer).

The essay deals in general with mental fatigue, its main physiological and psychological manifestations, with individual differences in these manifestations, and with fatigue in an extreme and abnormal degree. The psychology of work and fatigue is destined to play a most important part in industrial organization.

A report of a meeting of the *Société de Psychologie*, with a paper by G. Dumas on *Les mouvements volontaires*, is added, and also a report of a meeting of the *Société de Psychiatrie de Paris*, with an abstract of a paper by Mm. G. Maillard and Codet, on a *Diagnostic rétrospectif d'encéphalite épidémie avec sequelles psychiques*.

No. 5, May 1921.

Le Mouvement sans nerf (P. Wintsebert).

The paper presents an experimental study of primitive types of movement, chiefly in regard to the dual development of muscular and nervous systems.

Under the head of *Notes et Documents* L. Livet contributes a descriptive account of *Les rêves narcotiques et leur conséquences.* Reports of meetings of the Société de Psychologie contain papers by Ch. Lalo on *L'Art et la Vie*, by H. Wallon on *Psychonévroses et troubles d'origine sympathique*, and by Marcel Mauss on *L'expression obligatoire des sentiments*, a discussion dealing with certain of the oral ritual of Australian funeral ceremonies.

No. 6, June 1921.

La notion de partie chez l'enfant (J. Pieget).

The experiments show that the notion of 'part' is exceedingly ill-defined in young children. Its growth is traced, and its complex nature analysed.

La mémoire des noms propres et la fonction du réel (L. Dupuis).

Memory for proper names is a special practical application of verbal memory to purposes having a social value. It is found to a high degree only in persons who have a lively interest in their actual social environment.

A paper read to the Société de Psychologie by Étienne Rabaud entitled, *L'instinct maternel chez les mammifères*, and a sketch "*D'une généalogie des catégories esthétiques*," by G. Belot contributed to the same society are included.

The Psychological Monographs, Vol. XXIX

No. 1, whole No. 130.

A Qualitative Analysis of the Process of Forgetting (Harold R. Crosland).

Specific to forgetting are processes of 'typification' and 'analysis,' together with the mechanism of condensation, displacement or transposition, dramatization and secondary elaboration. In typification some facts stand out as central, the rest being dissociated from these, and forgotten, while in analysis the facts retained are selected and clarified. Interpolative or constructive processes in forgetting proceed much more slowly than destructive processes. Equally important are the mechanisms of certainty and uncertainty, acceptance and rejection. Affective factors appear to play comparatively little part in forgetting.

No. 2, whole No. 131.

The Growth of Intelligence (Edgar A. Doll).

The accepted theoretical curve of normal growth of intelligence misrepresents the probable facts. For practical purposes an empirical straight line curve of average normal growth in terms of relative units of mental age may be employed as a standard of comparison. Probably the adult level of intelligence is reached between the ages 13 and 14 years. Individual variability in the rate of intellectual growth of superior children is very marked. The contention that the intelligence quotient is approximately constant between the ages of 4 and 16 is not justified.

No. 3, whole No. 132.

Mental and Educational Measurements of the Deaf (Jeannette Chase Reamer).

A set of non-language mental tests, and an "educational survey test" were given in group form to 2500 deaf children. The non-language tests give normal distributions at each age from 8 to 21 +. The Educational tests give normal distributions from 10 to 18. Mixture of deaf and hearing children in the same school is markedly productive of bad results. The congenitally deaf are slightly below the adventitiously deaf both in mentality and in educational attainment. There is practically no difference between the partially and the totally deaf. Onset of deafness after six is less injurious to future educational progress than is deafness acquired before that age. The average difference in mental ability between hearing and deaf children is about two years, while for educational purposes the latter, as compared with the former are retarded by about five years, the discrepancy between the mental and educational retardation being due to the language handicap.

No. 4, whole No. 133.

A Socio-Psychological Study of Fifty-three Supernormal Children (William T. Root, Jr).

The results of a large number of different psychological tests applied to the group of superior children of varied age are described, and the consequently superior performances are analysed. Special attention is given to the effect of social factors,—the home influence, the child's social group, etc. Moral as well as intellectual factors are considered.

Beihefte zur Zeitschrift für angewandte Psychologie.

No. 23, 1920. Mk. 18.

Übungsfähigkeit und Übungsfestigkeit bei geistiger Arbeit (E. Gellhorn).

Three tasks were employed,—letter cancellation, multiplication and addition of single numbers. The practice curve for a given individual is found to be of the same form for all three tasks. Some persons show practice only during the work, some only after it, some both during and after the work. The latter show a want of uniformity in all three tasks. The course of the curve of retentivity of practice is generally the same for a given individual, but it differs widely in different people.

No. 24, 1920. Mk. 22.

Über die Fernald'sche Methode zur Prufüng des sittlichen Fuhlens und über ihre weitere Ausgestaltung (L. Jacobson-Lask).

An investigation upon children, youths and adults. The author concludes that with appropriate precautions and modifications this test can evaluate in young persons the ethical standard as effectually as the Binet-Simon tests can evaluate their intellectual standard.

No. 25, 1921. Mk. 28.

Zur Psychologie des Amputierten und seiner Prothese (David Katz).

An investigation into the illusions of those whose limbs have been amputated. The author studies the sensibility to touch, the localisation and the spatial threshold of the stump, and the differential sensibility for raised weights in the stump and in subjects provided with Sauerbruch's 'muscle canals.'

No. 26, 1921. Mk. 30.

Die Feststellung der psychischen Berufseignung und die Schule (Erich Stern).

The author insists that despite vocational advice the subject must believe that he has himself chosen his vocation. He analyses the material collected by the late Dr Minkus at Breslau concerning the written preferences of school-children for their future vocation, and his own material similarly obtained from Hamburg children, and he shows the insufficiency of the results unless supplemented by such specific questions as "Why do you choose that vocation?", "Who advised you to choose it?", "Which of these groups of vocation would you choose (unskilled, manual, office, business, personal, domestic, agricultural, artistic, higher professional)?", "What would be your order of preference of the following vocations within the particular group you have chosen?", "Are you good at such and such an occupation?", "Do you consider yourself well fitted for the occupation you prefer?" Questions were similarly asked of the teachers and parents of the children, and to the children were given certain group vocational tests.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETING

June 11, 1921. Nonsense Syllables and the Word Association Method of Jung, by R. J. Bartlett.

A preliminary Investigation into Suggestibility, by F. H. Cracknell.

July 23, 1921. Methods of Dream Analysis, by W. H. R. Rivers.

The Nature of Sentiments, by C. S. Myers.

(i) Some new Apparatus for Auditory Experiment; (ii) A portable Reaction time Apparatus; (iii) Tests of manual dexterity, by F. C. Bartlett.

Experiments on Aesthetic Appreciation, by L. Feasey.

(i) A Test of Muscular Precision; (ii) Motion study applied by Miss Bevington to the Lay-out of a Compositor's Case, by B. Muscio.

(i) Vocational Tests for Typewriting; (ii) the Smedley Dynamometer, by S. C. M. Sowton.

Psycho-galvanic Reactions in Asylum Patients, by E. Prideaux.

The Effects of Deprivation of Oxygen on Mental Efficiency, by J. P. Lowson.

(i) A Vindication of Helmholtz's Theory of Hearing; (ii) Colour Filters having Remarkable Properties, by H. Hartridge.

SECTIONAL MEETINGS

Education Section.

April 19, 1921. A National System of Education, by J. Clerk Maxwell Garnett.

May 24, 1921. The 'Ineffable' Affect of the Psycho-neurotic known as Anxiety and its bearings on the problems of Adolescence, by J. Turner.

June 8, 1921. Intelligence Tests, by Maria Montessori.

Medical Section.

April 27, 1921. Enforced Psycho-synthesis in certain cases of Analysis, by Paul Bousfield.

May 25, 1921. Emotion and Eye Symptoms, by W. Inman.

June 22, 1921. The Influence of the Endocrines in the Psychoneuroses, by W. Langdon Brown.

VOLUME XII DECEMBER, 1921 PART 3

THE NORTHUMBERLAND MENTAL TESTS.

BY GODFREY H. THOMSON.

(*From the Department of Education, Armstrong College, Newcastle-upon-Tyne.*)

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LIBRARY.

I. *Object of the tests* (pp. 201, 202).
II. *Preliminary investigations* (pp. 202, 203).
III. *The test booklet* (pp. 203–207).
IV. *The practice test* (p. 208).
V. *Marking the tests* (pp. 209, 210).
VI. *Norms of performance* (pp. 211–214).
VII. *Comparison with Stanford revision, etc.* (pp. 214–216).
VIII. *Distribution of the intelligence quotient in different types of school* (pp. 216–219).
IX. *Promotion within the elementary schools* (pp. 219–221).
X. *Conclusions* (pp. 221, 222).

I. OBJECT OF THE TESTS.

THE primary object of these tests was to discover gifted children worthy of free secondary education among those elementary schools of the County of Northumberland which had not this year sent in any candidates for the orthodox examination in English and Mathematics on which such free scholarships are usually awarded.

From one quarter to one third of the elementary schools of this county present no such candidates: and from year to year these are in the main the same schools, chiefly found in the remoter country districts. Northumberland is indeed rather sharply divided into a small densely populated district on the coalfield in the south east, and a large and very sparsely populated area comprising *inter alia* Tyndale, Redesdale, Coquetdale, all the Cheviots and Ottercaps, the Till and Breamish valleys, and many fell districts not in the Cheviots proper. The lack of candidates from the small schools in this area is presumably due in the main to (i) the difficulties of reaching a secondary school even if a scholarship were won and (ii) the difficulty of an isolated country schoolmaster or schoolmistress (teaching perhaps all the standards single handed) competing in the preparation of candidates with experienced heads of urban schools aided by a capable staff. It was felt that these difficulties could be in part overcome if some maintenance scholarships were given on a mental test which gave an equal or almost an equal chance to an unprepared candidate. Accordingly the County Education Authority, inspired mainly by Dr Andrew Messer of Lemington, offered twenty such scholarships (in addition to those given on the orthodox examination) open only to schools which had not competed in that examination, and asked me to prepare and to superintend a mental test for the candidates.

In order to gain a proper background for comparison, the mental test was to be given not only to the candidates who thus offered themselves, but also to whole schools and to certain whole districts as a survey, and finally also to a number of the already successful candidates on the orthodox examination. Thus there were nearly 3000 subjects for the tests, 414 of these being candidates for scholarships on the result of the tests.

II. PRELIMINARY INVESTIGATIONS.

As scholarships were to be awarded on the results, there was a strong feeling, shared both by myself and by Mr C. Williams, the Director of Education, that tests which were already public property could not be used. It was decided therefore to prepare and to standardise a test specially for this purpose, and it was planned to give Binet or other well-known tests ultimately, by way of check, to the best candidates. The preliminary work in connexion with the preparation of the tests was begun about Christmas 1920. I was indebted during its progress to a number of psychologists who aided me with suggestions, and to many teachers and headteachers of schools in distant parts of the United Kingdom, many being old pupils and old fellow students of my own[1]. In this way single tests, usually requiring ten minutes to be given to a group of 50 children, were tried in a rural county in the south, in a middle-sized mill town, in a cathedral city in the midlands, in the Channel Islands, in Scotland, in a large English city, and elsewhere, including one batch in America. Approximately 1000 children in all were subjects of some one or other of these preliminary trials, and each test was given usually to about 250 children.

By these preliminary trials the (for our purpose) less satisfactory tests were 'scrapped,' and gradual improvements were made in the survivors. The list of tests at Easter, 1921, was as follows:

---

[1] I am very grateful to all who helped and am only prevented from thanking these by name by their number and by the desire of some to remain anonymous.

Newly devised tests:
- (i) Hindustani Test.
- (ii) Extra Number Test.

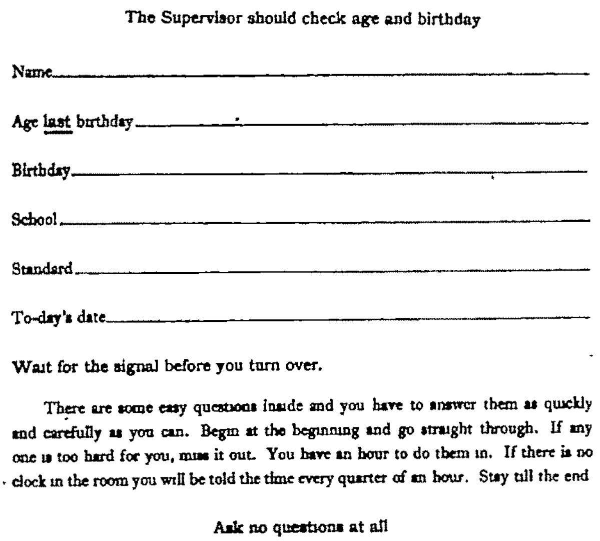
Tests in part new:
- (iii) Middle Word Test } both based on the work of Professor Stern of Hamburg
- (iv) Schema Test } but new in form of presentation.

Well-known forms of tests:
- (v) Extra Word Test.
- (vi) Number Series Test.
- (vii) Analogies Test.
- (viii) Opposites Test.
- (ix) Directions Test.
- (x) Completion Test.

Of these the first six ultimately formed the test proper. In thus choosing a large proportion of new tests I was actuated in part by a desire to make coaching for the tests almost impossible and in part, I confess, to a desire to make this experiment as original a contribution as possible. It appears to me very desirable that large numbers of new tests should be tried at every opportunity so as to increase our armoury.

III. THE TEST BOOKLET.

At Easter, 1921, a booklet of tests based on the above was put into type and between then and June 400 more children in various centres were tested by this booklet, in which only small changes were made. It is therefore given here in its final form (much reduced in size of print).



The Supervisor should check age and birthday

Name____________________________________

Age last birthday______________________________

Birthday__________________________________

School___________________________________

Standard__________________________________

To-day's date_______________________________

Wait for the signal before you turn over.

There are some easy questions inside and you have to answer them as quickly and carefully as you can. Begin at the beginning and go straight through. If any one is too hard for you, miss it out. You have an hour to do them in. If there is no clock in the room you will be told the time every quarter of an hour. Stay till the end

Ask no questions at all

TEST A1

In the tests given below you have to cross out the extra word in each line. For example look at this first line:

wood cork stone boat bladder

The extra word there is stone, because all the others float on water Look at this second set

chair table stool desk roof

The extra word there is roof because all the others are articles of furniture Now try to cross out the extra word in each of the remaining lines:

grapes oranges wool apple banana
rifle plough sword pistol lance

TEST A2

You have to give the number that comes next in each of the following lines of numbers The first three are answered for you to show what is meant:

1 2 3 4 5 6 (7)

Here the number that you have to write in the brackets is 7 because the first six numbers go up by steps of one at a time. The series below also have steps of different sizes but they do not always increase by adding the same number each time, but in other ways as well:

11 10 9 8 7 6 (5)

Here the numbers come down.

1 2 4 8 16 32 (64)

Here each number is twice as big as the one in front

Now try the others:

3 6 9 12 15 18 ( )
2 4 6 8 10 12 ( )
1 3 5 7 9 11 ( )
1 2 3 4 3 2 ( )

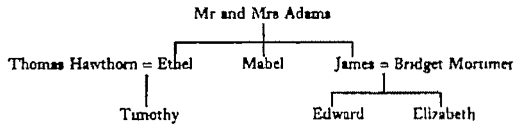
TEST A3

Read the following paragraph carefully and compare it with the diagram. The diagram gives the same facts as the paragraph.

PARAGRAPH

Mr and Mrs Adams had three children, (1) Ethel (the eldest), (2) Mabel and (3) James (the youngest). Ethel married Mr Thomas Hawthorn, and their son was named Timothy James married Miss Bridget Mortimer, and they had two children (1) Edward and (2) Elizabeth.

DIAGRAM



Now answer these questions (the first two are answered for you to show what is wanted)

	Answers
What relation is Timothy to Mabel?	Nephew
How many aunts has Edward?	Two
What is Timothy's surname?	
What is Elizabeth's surname?	
Who is Timothy's uncle?	

TEST A4

In your mind (without writing them down) you have to arrange the five words in each line below in their proper order, and then underline the middle word of this order. For example, consider

minute second year hour week

In their proper order these words would run thus · second, minute, hour, week, year. The middle word is hour and therefore hour is underlined above. Similarly shilling is the middle word of this next set

sixpence penny florin sovereign shilling

Now try to do the next tests in this way.

elephant sheep mouse cow puppy
forty thirty fifty ten twenty

TEST A5

You have to cross out the "extra" number on each of the following lines. For example, in

6 2 8 7 4 10

you cross out 7 because it is the only odd number, all the rest agree in being even numbers.

In this next case

4 7 8 6 19 3

you cross out 19 because it is the only number in double figures. Again in

14 3 15 9 6 12

you cross out 14 because all the rest are divisible by 3. Try to find and cross out plainly the "extra" number in each of the following lines

18 16 4 8 20 12
5 9 3 4 1 7
75 62 20 10 15 25
17 49 4 24 13 18

TEST A6

The sentences below are in a foreign language, and their meanings are given in English. In each English sentence a word is underlined, and you have to underline the word which corresponds to it in the foreign sentence. You can do this by comparing the sentences with each other. For example, look at these:

1 Kuchh malai	some cream.
2. Kuchh puri leoge	will you take some cake?
3 Misri leoge	will you take sugar?

By comparing 1 and 2 you see that kuchh must mean some because it occurs in both sentences. Underline it in sentence 1 before you go any further. By comparing 2 and 3 you see that leoge means will you take. Underline it in 3. Then the only word you do not know in 2 is puri which must mean cake, so underline it in 2. You have now underlined in each foreign sentence the word corresponding to the underlined part of the English sentence.

Notice that the foreign words are not always in the same order as the English words.

Underline your words plainly.

You have not to write anything, only to underline the proper words

Now try these

Ek piyala chae	A cup of tea.
Yih chae bahut achchhi hai	This is very good tea.
Chae bilkull taiyar hai	Tea is quite ready.
Kab taiyar karoge?	When shall you make ready?
Main bahut pyasa hun	I am very thirsty.
Bahut achchhi hai	It is very good.
Yih mera rumal nahin hai	This is not my handkerchief.

(You may turn back to read examples if you wish to do so.)

TEST B 1

Cross out plainly the "extra" word in each of the following lines:

charity	kindness	benevolence	revenge	love
square	circular	oblong	hexagonal	triangular
needle	tack	nail	knife	pin
coal	bread	coke	wood	paper
bran	wool	cotton	hemp	jute
hair	feathers	wool	grass	fur

TEST B 2

Give the number that comes next in each of the following lines:

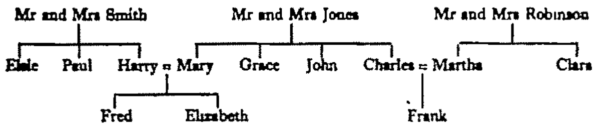
1	3	9	27	81	243	( )
17	15	18	11	9	7	( )
1	2	4	7	11	16	( )
96	48	24	12	6	3	( )
7	5	5	7	5	5	( )
9	8	7	6	7	8	( )

TEST B 3

Cross out the "extra" number in each of the following lines:

26	3	7	31	13	17
18	22	30	24	6	12
81	27	11	1	9	3
8	2	16	32	6	4
7	28	21	35	14	27

TEST B 4



From the above diagram answer the following questions:

	Answers
What is Frank's surname?	
What relation is Elizabeth to John?	
What relation is Elizabeth to Frank?	
What relation is Elizabeth to old Mr Jones?	
How many aunts has Frank?	
How many uncles has Fred?	
Who are Frank's grandparents?	

TEST B5

PART 1

In your mind (without writing them down) you have to arrange the five words in each line below in the proper order and then underline the middle word of this order:

paragraph	book	chapter	sentence	word
house	street	room	town	county
orange	red-currant	grape	plum	melon
general	sergeant	captain	private	corporal

PART 2

In the remaining line two such sets of five words are mixed up together. You have to separate them mentally, arrange them in order, and underline the middle word of each. Do not write them down, just underline the proper two words.

leaf, leafvein, twig, sowing, reaping, thrashing, branch, ploughing, tree, baking.

TEST B6

In each foreign sentence underline the word which corresponds to the underlined word in the English sentence.

Underline your words plainly. You have not to write anything, only to underline the proper words.

Mausam badalta hai	It is changeable weather.
Achchha khub mausam hai	It is good weather.
Sirf das baje hai	It is only ten o'clock
Das gaz napo	Measure ten yards.
Ham kahan jaen?	Where shall we go?
Ham chha baje khama khawenge	We shall dine at six o'clock
Yih rah kahan jati hai?	Where does this road lead to?

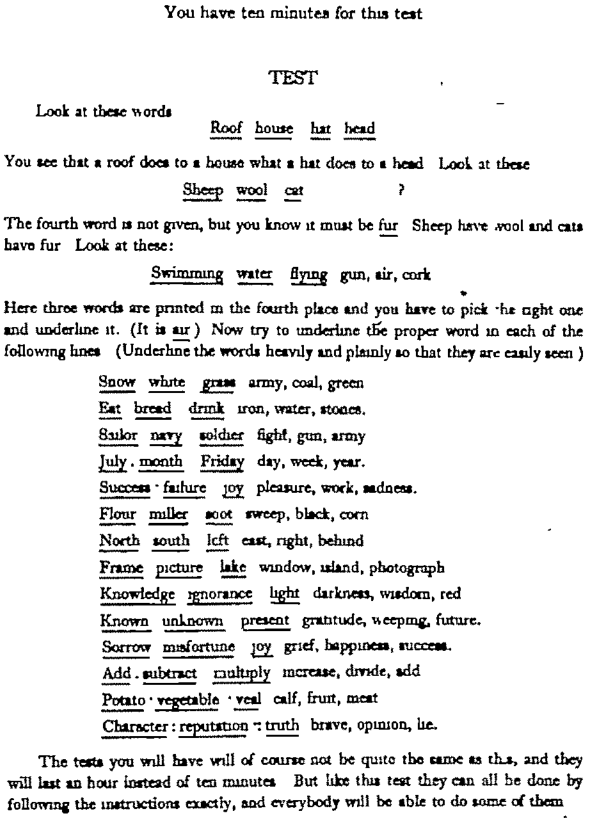
Look over the tests again, until time is up.

One principle which guided the formation of this booklet was that of leaving little or nothing to the Supervisors, who, it will be seen, are merely required to have the front page filled in and then to see that the children have exactly one hour in which to attempt the questions. The tests were given in the children's own schools, and supervised by their own teachers. It was felt by many that in these circumstances the fairest method was one which eliminated the untrained supervisor as much as possible; for there might be differences in the ability to give the tests verbally, and some teachers might even, all unconsciously, give more help than others.

Another principle was the avoidance of any emphasis on speed. It will be found that practically all children tested can finish all they can do in the hour.

IV. THE PRACTICE TEST.

Fears were expressed by some with whom the tests were discussed that certain highly-strung children might by the very novelty of the tests be thrown into a dazed condition and be totally unable to do themselves justice. It may be said at once that none of the teachers who gave the tests in the preliminary investigations reported anything of this sort whatsoever. On the contrary, it was universally reported that the pupils thoroughly enjoyed the tests. As a further safeguard, however, against undue shock from novelty, and as a preliminary practice, the following ten minutes' test was devised. It was given on the day preceding the test proper except in two schools where the parcel did not arrive in time.



You have ten minutes for this test

TEST

Look at these words

Roof house hat head

You see that a roof does to a house what a hat does to a head Look at these

Sheep wool cat ?

The fourth word is not given, but you know it must be fur Sheep have wool and cats have fur Look at these:

Swimming water flying gun, air, cork

Here three words are printed in the fourth place and you have to pick the right one and underline it. (It is air) Now try to underline the proper word in each of the following lines (Underline the words heavily and plainly so that they are easily seen)

Snow white grass army, coal, green
Eat bread drink iron, water, stones.
Sailor navy soldier fight, gun, army
July . month Friday day, week, year.
Success · failure joy pleasure, work, sadness.
Flour miller soot sweep, black, corn
North south left east, right, behind
Frame picture lake window, island, photograph
Knowledge ignorance light darkness, wisdom, red
Known unknown present gratitude, weeping, future.
Sorrow misfortune joy grief, happiness, success.
Add . subtract multiply increase, divide, add
Potato · vegetable · veal calf, fruit, meat
Character : reputation :: truth brave, opinion, lie.

The tests you will have will of course not be quite the same as this, and they will last an hour instead of ten minutes But like this test they can all be done by following the instructions exactly, and everybody will be able to do some of them

V. MARKING THE TESTS.

The rule for marking the tests was in general that only the *exact* answer gained points. This requires interpretation in several cases, however, as given below.

There are 60 items in the tests in all, and for simplicity the marking was made quite straightforward and the score was the number of items correct.

The following rules will appear arbitrary in some instances but they are based on an examination of a large number of cases and appear to be more justifiable than other alternatives. It must be remembered that all the tests are in part directions tests; disobeying directions should therefore lose points even if the answer is intelligible.

*Test A*1. Correct answers *wool, plough,* 2 points. No point if two words in a line are underlined, or if the words are written down, or if all the other words are crossed out and the proper one left. Underlining or ringing the word is accepted as crossing out. Alterations are allowed if they are perfectly clear. *These principles apply* mutatis mutandis *in the other tests.*

*Test A*2. Correct answers 21, 14, 13, 1. 4 points. The point is awarded also if the series is further continued correctly.

*Test A*3. Correct answers *Hawthorn* (or Timothy Hawthorn, or Mr Hawthorn, but *not* Thomas Hawthorn); *Adams* (or Elizabeth Adams); *James* (or James Adams, but *not* James Mortimer, or Mr Adams, or J. B. Mortimer). 3 points. Spelling mistakes are ignored if the meaning can be grasped.

*Test A*4. Correct answers, *sheep, thirty.* 2 points.

*Test A*5. Correct answers 18, 4, 62, 4. 4 points (4 is the only number not in double figures).

*Test A*6. The language is a simplified Hindustani. Correct answers *chae, hai, bilkull, taiyar, bahut, achchhi, yih.* 7 points. No points are deducted if words in the English are unnecessarily underlined (such candidates seldom score more than a chance point in the seven). Marks are neither given nor deducted for the explanatory sentences (kuchh etc.).

*Test B*1. Revenge, circular, knife, bread, bran, grass. 6 points.

*Test B*2. 729, 5, 22, 1½, 7, 9. 6 points. No point for 1 instead of 1½.

*Test B*3. 26, 22, 11, 6, 27. 5 points.

*Test B*4. Jones,
niece (*not* uncle)
cousin (or cousins, *not* half-cousin)
granddaughter (or grandchild, *not* grandfather)
three
three
Mr and Mrs Jones and Mr and Mrs Robinson. 8 points, *i.e.*, 2 for the grandparents of Frank, thus:

one grandparent only	no point.
any 2 or 3 grandparents	1 ,,

4 grandparents 2 points.
too many grandparents no „
Robinsons = two grandparents.
Jones and Robinsons = 4 grandparents.
Jones and Robinson = 2 „
Jones (alone) = no point.

Test B5. paragraph, street, plum, sergeant, twig, reaping. 6 points.
Test B6. badalta, mausam, sirf, das, ham, baje, kahan. 7 points.

The marking was done by a team of 25 of my students, under my direction. Of these students 10 were graduates of a year's standing, the remainder undergraduates of two years' residence. All had had a course of lectures on mental tests. I also had the assistance of the staff of my department, Miss Melville, Miss Bailes and Mr Vernon Brown. To all of these my sincere thanks are due.

The tests were worked on Friday June 18th. They were received by us on Monday forenoon, and we began work at 2.30 on that day. The marking, checking, and statistical work was all completed and the Intelligence Quotients entered on the lists by 5 p.m. on Wednesday, after 13½ hours' work.

Each student was supplied with scored copies of the booklet for marking, and with ready reckoners:

(1) To enable the child's age in months to be read off from age last birthday and month of birth.

(2) To enable the mental age to be read off from the score in points (these were prepared after the first marking had been completed).

(3) To assist in performing the division of mental age by actual age so as to obtain the Intelligence Quotient.

These last ready reckoners were based on the fact that the actual ages ranged from about 100 months to 160 months. In the fraction

$$\frac{\text{Mental Age}}{\text{Actual Age}}$$

if the divisor was from 100 to 110, the I.Q. (Intelligence Quotient) was taken approximately as 100 + difference between numerator and denominator.

From 111 to 139, 100 + ⅚ difference.
From 140 onwards, 100 + ⅔ difference.

The general rule was made that all high I.Qs. (above 120) and all low (below 80) were to be exactly calculated by two of us who were expert with slide rules.

years in advance of their proper age. Such children are not represented in the 13½ year old line or the 14½ year old line.

After various mathematical manipulations of Table I which need not detain us here, and which may be described as 'smoothing the surface' which that Table represents, I adopted the following norms of performance:

9½	10½	11½	12½	13½	years
9	15	24	33	42	points.

These points are shown in Fig. 1, where they are joined by a smooth curve which is continued outside these limits so as to form an 'ogive,' the shape which both from theoretical reasons and from some of the preliminary investigations appears the most probable. From this curve the mental age corresponding to each point score can be read off. To facilitate this however the following ready reckoner was prepared.

TABLE III. *Northumberland Norms**.

Marks	Age in months	Marks	Age in months	Marks	Age in months	Marks	Age in months
60	198	44	164	28	144	12	120
59	195	43	163	27	142	11	118
58	192	42	162	26	141	10	116
57	190	41	161	25	140	9	114
56	188	40	160	24	138	8	112
55	186	39	158	23	137	7	110
54	184	38	157	22	136	6	108
53	182	37	156	21	134	5	105
52	180	36	154	20	133	4	102
51	178	35	153	19	132	3	98
50	176	34	152	18	130	2	94
49	174	33	150	17	129	1	89
48	172	32	149	16	128	0	84
47	170	31	148	15	126		
46	168	30	146	14	124		
45	166	29	145	13	122		

* In the case of children who obtain more than 52 marks (mental age 15 years) or less than 9 marks (mental age 9½ years) a harder or an easier test, respectively, is desirable, and the I.Q. calculated from the above norms should, in these cases, be regarded as uncertain, in most cases probably too low. For since the highest mental age obtainable on these norms is 16½, clever children of 12 and over are handicapped by the fact that a very high I.Q. is not arithmetically possible for them, while below a certain age the reading ability required may not be present. The scholarship candidates were all below 12½, but as a safeguard all cases of high marks were separately considered even if the I.Q. was not very high. Even in the full Binet tests the clever older children do not get full scope (cf. Terman, *Intelligence of School Children*, p. 191 footnote).

It must be clearly understood that in its lowest and uppermost reaches this ready reckoner is based upon extra-polation: and in any case probably no very definite mental age can be assigned to the very lowest marks from 0 to 10 (see also footnote to the table). But to facilitate the working of the team of markers it was found better actually to assign mental ages to these very low marks and to carry on the routine of calculation.

The mental age thus found, divided by the actual age (also in months) gave an Intelligence Quotient, based however on these tests alone and not to be too readily assumed to be equivalent to a proper Binet I.Q. The comparison with Binet I.Qs. is still going on, but may be in part described in the following paragraph.

VII. COMPARISON WITH STANFORD REVISION, ETC.

The first comparison with the Stanford-Binet-Simon tests was possible during the preliminary investigations owing to the kind co-operation of one of H.M. Inspectors of Schools in the large city already referred to, who had tested by the Stanford Revision a large number of children during the preceding year. To 50 of these the Northumberland Group Test was given, and a correlation with the Binet I.Qs. of about 0·8 was found.

Binet testing of samples of the children is planned for next winter and has indeed already been begun, showing so far a satisfactory correlation.

Of fourteen candidates from rural schools who will be granted maintenance scholarships, eight gave the following comparison:

Index No. of candidate	I.Q. on Northumberland Group Test	Binet I.Q.
148 Y.J.	146	151
72 G.J.	137	134
134 N.E.M	133	135
2647 T.E.I.	133	120
38 G.J.E.	130	127
2420 I.F.W.	129	128
2741 P.E.T.	128	123
60 G.K.	128	105

These were given a full test by Terman's Revision of the Binet scale. Half of them were tested by Mr C. A. Richardson, H.M.I., and half by myself. The only serious discrepancy is the last case, in which in the Binet test there was failure in tests involving imagery of a kind not needed in the group test. The remaining six cases could not be given a full test for lack of time. Two were given a full test which had to be broken off. They gave these results:

Candidate	Northumberland I.Q.	Binet I.Q.
150 M.E.	141	greater than 120
56 S.R.	122	„ „ 109

Four were given a short test of three questions for each year (the questions carrying double the credit in months of the Terman scale). This took only 25 minutes and probably involves a good deal less strain than the full test, so that these candidates' Binet I.Qs. are possibly too high.

Candidate	Northumberland I.Q.	Binet I.Q.
74 O.B.S.	139	137
85 K.A.	133	145
58 B.T.H.	129	135
71 B.C.N.	129	149

In a rather unsatisfactory cyclostyled form the tests have been given (in addition to the elementary school children) to 104 boys in a Preparatory School, varying in actual age from 8 years 4 months to 13 years 11 months. Of these, 32 had on some previous occasion been given a full Binet test by the headmaster, working only from the instructions given in Terman's book. For these 32 cases the correlation between the Northumberland I.Qs. and the Binet I.Qs. is 0·70 ± 0·07. If, as is recommended in the footnote to the table of norms, all cases are excluded of mental ages greater than 15 years or less than 9½ years, there remain 20 cases for which the correlation has risen to 0·83 ± 0·05. They are given below:

Northumberland	Binet	Northumberland	Binet
133	130	113	106
125	113	112	111
124	120	109	98
123	120	106	114
121	136	103	98
118	132	97	89
116	137	92	97
115	121	91	84
115	116	90	90
114	122	84	93

The average Northumberland I.Q. is 110 for these cases, the average Binet I.Q. is 111, the average difference regardless of sign is 7½ points. (The probable error of a Binet test is given by Terman as 4½ points.)

An immediate mass comparison is possible in the following way. In the Northumberland 2710 cases the Test I.Qs. are distributed as follows:

Above 140	0·3 per cent.
131 to 140	2·9 „ „
121 „ 130	8·6 „ „
111 „ 120	15·2 „ „
101 „ 110	22·4 „ „
91 „ 100	23·9 „ „
81 „ 90	15·2 „ „
71 „ 80	8·8 „ „
61 „ 70	2·4 „ „
60 and below	0·3 „ „

This distribution when compared with that given by Terman in Figure 2 on page 66 of *The Measurement of Intelligence*, is seen to be rather more widely spread. The quartiles of Terman's distribution are about 91 and 109, those of the Northumberland distribution 89 and 111.

Detailed correlations of the Test I.Q. with success in the orthodox examination have not yet been fully worked out. But a remarkably high correlation exists, and is obvious on looking over the lists, between the I.Qs. and the head-teachers' opinion of very bright and very slow children. Whenever on the lists a child is noted as 'very bright,' or 'already success~~ful in scholarship examin~~ation,' the I.Q. is high and the opposite is the case with defective and specially slow children.

The single candidates submitted for scholarships on the test from remote country schools formed a group very noticeably in advance of the average. The highest I.Q. found (174, the next highest being 153) was of an eight year old boy in a small border village in the heart of the Cheviots, close to the source of the North Tyne.

VIII. DISTRIBUTION OF THE INTELLIGENCE QUOTIENTS IN DIFFERENT TYPES OF SCHOOL.

It is of interest to inquire whether there are any differences in the results from town schools, small country schools, schools in mining centres, etc., and also whether other differences can be seen between schools in poor and in well-to-do districts.

It must at once be admitted that no clear picture can be obtained from the present investigation, although some points are strongly suggested. A true account of the distribution of the Intelligence Quotients as between school and school could only be given had the whole child population of each school entered for the test, and this was not the case. Those schools which only entered candidates whose chances of doing well appeared to the teachers to be good, will naturally have better results than others where every child attempted the tests.

The following Table IV, and figures 2 and 3 enable a comparison to be made between 17 typical schools in which it would seem from a comparison of statistics that a large proportion of older children were tested.

The suggestion which these figures bring with them is that the most intelligent children are to be found in the Cheviots, where the results are even better than in a well-to-do town suburb. It was mentioned earlier in this paper that preliminary experiments in other counties of England had shown that children in a large city were at least a year in advance of those in a very rural environment. But in that case there were, there can be no doubt, great differences of race and social standing, and the city and the rural district were in entirely different parts of England. Here in Northumberland the population is probably fairly homogeneous as regards race, compared with many other counties, and the Cheviot children are to a large extent the descendants not so much of farm labourers as of Border troopers. The old family names are still the most numerous.

TABLE IV. Various Types of School.

School	Number of cases	Percentage over I.Q. 100	Type of school
A	25	80	Small country schools in the Cheviot valleys
B	17	70	
C	22	50	
D	380	46	Four schools in a moderate sized town
E	91	39	Two schools in a small market town
F	129	45	Four schools in large mining villages
G	128	51	
H	111	32	
I	112	28	
J	141	29	Two schools in a poor suburb of Newcastle
K	63	35	
L	101	55	Two schools in a well-to-do suburb of Newcastle
M	82	56	

Note. Newcastle schools themselves are not under the county.

Miss M. E. Bickersteth's careful inquiry into the mental ability of children in the Yorkshire Dales[1] is a model of what ought to be done in Northumberland. In this present connexion she says: "Some of the schools were only reached after a walk of many miles over the fells, and...at these schools, far from any village, and attended by children living in lonely farms on the moors, sometimes three or four miles from the school, the performance at the tests was almost invariably above the average for the Dales as a whole. In these remoter Dales 84 % of the children were above the average for the Dales as a whole in the reasoning test, and 74 % in the Memory Test, while very marked ability was not infrequently shown in the case of individual children." This verdict could be given almost unchanged as regards the Northumberland country schools, did our rough and ready experiment justify such a definite conclusion. It seems plausible to assume that the draining away of talent to the towns has not touched the most remote spots.

The difference between the poor and well-to-do districts, both equally near Newcastle and approximately equally dense in population, is more certain. Whether it be a real difference in intelligence or not is unproven, but in my own opinion it is probably real in great measure, though some allowance must be made for vocabulary and dialect difficulties. A comparison with the Preparatory School referred to above (in comparing Northumberland and Binet I.Qs.), is not allowable, for this school has a high reputation and a long waiting list, and its pupils are selected, not representative, members of the wealthier classes, which sufficiently explains the high average I.Q.

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[1] This *Journal*, 1917, IX, 23.

IX. PROMOTION WITHIN THE ELEMENTARY SCHOOLS.

During the preliminary experiments (not in Northumberland) it was noticed that, when tabular s[illegible]ents of the results were drawn up in the same form as Table I, in which the cases are separated both as regards age and as regards 'standard' or form in the school, differences between different schools could be observed suggesting that in some promotion was more elastic than in others. Consider for example the following tabulation of 49 cases from a large manufacturing and business city:

TABLE V, showing average marks in a special case.
49 cases in a large city, not in Northumberland
(full test in almost the final form).

	School Standard				
Age	III	IV	V	VI	VII
13½	—	—	25·7	34·8	**34.0**
12½	—	2·0	26·7	**41.4**	—
11½	11·0	21·7	**36.2**	—	—
10½	—	**25.2**	38·0	—	—
Northumberland norms	9	15	24	33	42

Here it is noticeable that not only do the averages naturally increase in each row from left to right (meaning that boys of the same age in a higher standard do better than those in a lower standard), but also the averages increase from above downwards in each column, meaning that the youngest children in each standard are also the best. This at once suggests the possibility that promotion by ability has not been as free as it might well be. As has been said, this phenomenon was found in some schools and not in others. Very occasionally there were signs of the reverse process, indicating premature promotion, but the type shown in the last table was the most common.

If we now consider the Northumberland results in Table I in the light of this suggestion, we see that there is no steady increase or decrease in the columns; the figures are, within reasonable limits, constant or vary irregularly. This suggests that promotion is well in accord with ability, and indeed it might be expected that in small schools this ideal would be attained more readily than in large schools, for in the former, promotion to a higher standard may not involve any change of seat or of teacher, whereas, in the latter, promotion is often prevented because of difficulties of classroom accommodation, each class having 60 seats which must be kept full, and being unable to increase beyond that number.

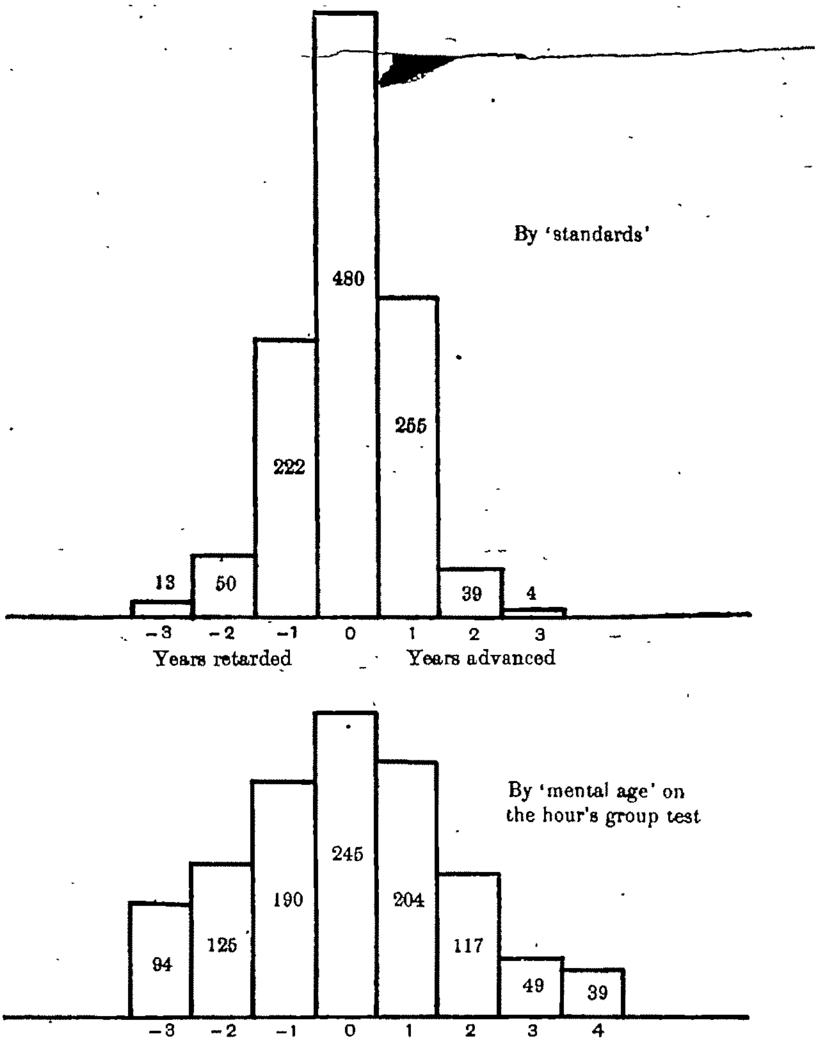

Fig. 4. Advanced and retarded children of 11 years of age.

It may be, however, that the Northumberland averages do nevertheless hide discrepancies of promotion; this possibility is strengthened by the following considerations. Figure 4 shows the distribution of the 1063 children, of 11 years last birthday, who took the test, arranged according to their standard. A similar scatter diagram on the basis of mental age is shown in the lower part of the same figure.

It is seen at a glance that the scatter by standards is much less than the scatter by mental age, although both centre round the same point. It would seem that there is, in spite of our previous argument, an undue reluctance to move a boy either above or below his proper age standard, though the average class ability is kept right. We must of course bear in mind that other factors as well as intelligence are rightly considered in promoting or retarding a boy.

Further analysis of the figures suggests that there is considerable difference, as between school and school, in the freedom with which promotion is given to ability. The figures for 104 cases from the moderate sized town (represented in Fig. 2 by D) suggest that here greater freedom of promotion is desirable. They are given in Table VI:

TABLE VI. 104 cases from a moderate sized Northumbrian Town.
Average marks.

	Standards				
Age	III	IV	V	VI	VII
13½	—	—	—	19·0	—
12½	—	2·3	16·3	**30.8**	44·7
11½	2·0	9·6	**20.1**	38·6	—
10½	—	**19.0**	38·0	—	—
Whole Northumberland	9	15	24	33	42

Here in each standard the youngest children are distinctly the best.

X. CONCLUSIONS.

The experiment which has been described is still in progress. But results so far seem to suggest that intelligence tests may assist Education Authorities (1) in gauging the differences in average intelligence in different parts of their area and in different types of schools, (2) in selecting children for higher education even from schools where remoteness or other factors are a handicap in the academic type of examination, (3) in drawing conclusions as to the freedom of promotion in different schools and (4) in comparing their area with other areas in all these matters.

The correlation of the I.Qs. found by this group test, with full Binet I.Qs., appears to be great enough to make the former a useful guide, and it would be possible without undue expense and time to obtain an I.Q. for every child over 9 in the county within a month.

The distribution of intelligence suggested by the tests is such that the highest ability appears to be found *close to the* cities and *far away* from the cities, the intermediate areas having fewer cases of high ability, as though they were drained by selection.

But all these tentative conclusions must be accepted only with the greatest caution, and the observation of the selected candidates and others with known I.Qs. in their secondary schools, may modify them. Numerous other points of interest also arise, among which may be mentioned the correlation of the different types of test (*e.g.* the Hindustani test, or the Number Series test) with success afterwards in different branches of school work.

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A STUDY ON THE PERCEPTION OF FORM.

By ARTHUR RAGNAR GRANIT.

(*From the Helsingfors Physiological Institute.*)

1. *The perception of lines* (pp. 223–227).
2. *Methods of experiments employed* (pp. 227, 228).
3. *Experiments on children* (pp. 228–237).
4. *Experiments on adults* (pp. 237–240).
5. *The construction of forms* (pp. 240–247).

I.

According to Bühler[1], we may in our perceptions of lines distinguish the following variable elements, impressions (i) of rectilinearity or curvature, (ii) of magnitude, (iii) of direction (p. 69). Bühler also gives an elaborate analysis of these perceptions. After having on the basis of exact experiments pointed out the extreme accuracy of our perceptions of rectilinearity, he criticises earlier theories concerning it and arrives at the following hypothesis; "Certain series of retinal elements are marked by a connexion (original or acquired) of the elements with one another. If these are stimulated simultaneously we receive the impression of a straight line" (p. 86). Only, he believed, by assuming in the organ of sight itself a receptive mechanism of this description can we explain our extraordinary susceptibility to deviations from rectilinearity. But this hypothesis cannot explain the perception of curvature (p. 115). We have well nigh innumerably different perceptions of curved lines. A corresponding connexion of retinal elements in this case is out of the question. Bühler emphasises that he is here advancing upon unbeaten paths, and resorts to an analogy with the perception of depth. Both kinds of perception are direct, but nevertheless seem to be caused by many different factors (p. 114). Yet it seems to me that every hypothesis concerning the perception of lines ought principally to keep to the purely linear character that belongs to them all. Besides, Bühler's theory of rectilinearity is liable to the same objection that he himself raises against a similar explanation of the perception of curvature. For we perceive straight lines under every conceivable aspect and in every direction.

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[1] K. Bühler, *Die Gestaltwahrnehmungen*, Stuttgart, 1913.

But where on the retina is it possible to imagine so grea [illegible] "series of retinal elements" that they can be supposed to [illegible] all the different directions, in which the straight lines run? [illegible] Bühler is in this case faced with the same difficulty that pr[illegible] from applying his theory to the perceptions of curvature.

But if we make the linear factor, common to both kinds of i[illegible] our starting point, the problem assumes a different aspect. [illegible] periments made during the war by Goldstein and Gelb[1] on [illegible] injured in the back of his head, may be mentioned in this co[illegible] Their patient was unable to perceive lines. The experimenters soon observed that when reading, he used to move his hand as if to follow the written text. He followed print with peculiar movements of the head. The patient was so dependent on these auxiliary movements that, if a written word was crossed over with a few strokes, he was unable to read it. He did not know what lines he was to follow. He behaved in the same manner when confronted with other visual perceptions of form. Goldstein and Gelb tried to isolate the patient's visual perceptions. For this purpose they made some experiments with negative after-images. The patient was told to look fixedly at a strongly lighted triangle and describe his impression. Then the stimulus was removed and he was again asked to describe what he saw. Now it was found that the patient had in the former case seen a triangle, but the after-image he describes in the following way: "It is neither round nor angular; I cannot describe it in any other way" (p. 68). The experimenters were also able to ascertain that he could not distinguish a circle from a triangle if he was ordered to look fixedly at their centres (p. 74). The patient thus had primary visual impressions of surfaces, but could not perceive contours. Goldstein and Gelb's explanation of the phenomenon above described is founded on those theories according to which there are special forms of synthesis or combination (*Gestaltqualität*)[2]. Thus they maintain that their patient lacked those forms of synthesis that are called rectilinearity and curvature.

What then does this 'explanation' of the perceptions of rectilinearity and curvature imply? I call attention to the vague concepts that are current in the literature on this subject. Not even the fundamental term 'Gestaltqualität' has so far been sufficiently defined. This word is used as a common appellation for a number of psychological factors that do not admit of being included within any one system. And yet it seems possible to find a common criterion for all forms of synthesis—*the synthetical impressions cannot be ranged immediately with the sensations and are probably rooted in specifically central processes.* From this point of view the perceptions of lines must be defined as the psychic results of a central psycho-physiological process.

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[1] Goldstein und Gelb, "Psychologische Analyse hirnpatologischer Fälle. I." *Ztschr. f. d. ges. Neurol. u. Psychiat.*, 1918, XLI.

[2] The term is translated according to E. B. Titchener, *A Text-book of Psychology*, New York, 1912, 371–73.

But Goldstein and Gelb seem to have overlooked the points of contact that exist between their investigations and the phenomena of simultaneous contrast. Their patient, let us remember, could not perceive distinct contours in isolated visual impressions. Now it seems most natural to assume that the man suffered from disturbances in the process to which simultaneous contrast is due. Recent investigations[1] have shown the central nature of this process. The contrast can be produced binocularly by means of suitable arrangements, when every possibility of a reciprocal action of different retinal elements is excluded. I think it extremely probably that the same central psycho-physiological process underlies simultaneous contrast and the purely linear character peculiar to the perceptions of lines and contours. A more exact description of the nature of our impressions of rectilinearity and curvature cannot, I think, be formulated by science at present. It is to be observed that this hypothesis cannot show to what factor the distinction between the perceptions of a straight line and a curve is due.

There are several analogous psycho-physiological processes, *e.g.* the filling out of the blind spot. Wertheimer[2], in his important inquiry on our visual perception of motion, seems to regard this as due to some species of 'cross-functions' (*Querfunktionen*) in the higher centres. Whether, at present, so explicit a physiological theory is justifiable or not, must be left to future research to decide. We will content ourselves with recognising that Wertheimer's experiments have shown that there is a specifically central process underlying our visual perceptions of motion. Goldstein and Gelb[3] also mention the connexion between the perceptions of lines and of motion. Some experiments that they made on their patient clearly showed that he could not perceive motion, but only a succession of stages which led him to the conclusion that motion had taken place (p. 92). This is in all respects a very remarkable corroboration.

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[1] Köllner, quoted according to M. v. Frey, *Vorlesungen über Physiologie*, Berlin, 1920, 363.

[2] M. Wertheimer, "Experimentelle Studien über das Sehen von Bevegung." *Ztschr. f. Psychol.* 1912, LXI.

[3] *Loc. cit.*

It is not altogether impossible that certain other phenomena may be referred to the same category. One meets with a similar line of thought in the writings of Watt, who has used for these phenomena the term 'integration.' Without feeling in any way obliged to accept Watt's[1] wide application of this term, we will provisionally use it in the following general sense: *The integrations are direct impressions due to psycho-physiological processes of specifically central nature. In relation to the sensations they are secondary elaborations*[2], *and thus they seem to be expressions of certain functions peculiar to the higher centres of consciousness, an addition to our sensations.* In the introduction to his first article on integration Watt[3] points out that the intimacy of connexion between nerve-paths or impulses emanating from different sense-organs has been somewhat exclusively considered to consist in the mere coordination or association of afferent or efferent impulses with one another. "Sufficient attention has hardly been paid to the possibility that upon these afferent impulses an afferent structure might be raised which is dependent upon, but essentially an addition to these. To distinguish it from mere coordination such a structure might well be called integration." Such integrations are, *e.g.*, the perceptions of depth, distance and motion. Melody Watt[4] regards as auditory motion, *i.e.* an integration.

After this digression we will return to the perceptions of lines and supplement what has been said with some purely theoretical speculations. Let us picture to ourselves a dark rectangle on a white surface. Simultaneous contrast causes the rectangle to appear sharply defined against the white background. Its contour has a certain marginal function. If we further imagine that the short sides are steadily shortened while the longer sides, unaltered, approach one another, the rectangle becomes narrower and narrower until at last the rectangle has passed over into a straight line. An analogous view might, without difficulty, be applied to curves, but straight lines are from a practical standpoint more important.

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[1] Henry J. Watt, "The Elements of Experience and their Integration: or Modalism." This *Journal*, 1911, IV. 127–204.

[2] This was observed in Goldstein and Gelb's patient who had primary visual sensations but lacked the integrations built on these, *i.e.* perceptions of motion and contour.

[3] Henry J. Watt, "Some Problems of Sensory Integration." This *Journal*, 1910, III 323–347.

[4] Henry J. Watt, "The Elements of Experience and their Integration: or Modalism." This *Journal*, 1911, IV. 127–204.

With the perceptions of lines Bühler has associated the perception of magnitude and of direction. A complete description of a line requires, of course, these attributes; but nevertheless, they seem to be isolated in a degree which justifies an independent treatment of them. In connexion with the perception of proportion we will later on briefly discuss the perception of magnitude and of direction.

II.

In the perception of lines we thus see the effects of the integrative process that has been briefly described on the preceding pages. The resulting integration, the surface and the contour or the isolated line, underlie all perceptions of form. But form is difficult to isolate: it belongs to the whole complex. Just as we form and divide time in rhythm, so we construct our primary perceptions of space according to definite laws. This 'construction' gives them form. If we, therefore, try to understand the essence of form, we can do this only by finding its laws. These are most clearly formulated in the imitative arts. Accordingly we are led to the study of the laws of composition in painting and sculpture.

But we are also free to adopt another manner of proceeding. We may examine form, as it were, *in statu nascendi*, by showing some more or less formless[1] figures in order to observe according to what principles the construction takes place. We may take such experiments on persons of different ages in order to find out whether the laws are constant or variable in the course of individual development. We may, in other words, treat the problem from a psycho-genetic point of view. The present investigation offers an attempt in this direction.

In accordance with the principles set forth above, the experiments are intended to answer the general question, How do individuals in different stages of development construct figures composed of surfaces or lines?

The figures used (Fig. 1) were of three kinds: (i) Figures drawn to resemble some familiar object—A-figures; (ii) simple figures without any direct resemblance to familiar objects—B-figures; (iii) complex figures, drawn without any plan—C-figures.

The size of the figures was about 1—1·5 cm. They were drawn with black ink on unpolished glass-plates. The experiments were conducted by means of a tachistoscope, in a room with subdued light, the time of exposure was 100σ. Each exposure was preceded by the call 'ready,' which was to warn the observer to concentrate his attention and to gaze at the figure in the tachistoscope, dimly visible in the middle of the evenly dark plate. A somewhat shorter time of exposure was sufficient for adults while the children required about twice the time above mentioned. Where a longer time was necessary it was given by doubling the number of exposures.

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[1] The formlessness in this case is due to absence of similarity to familiar forms. Absolute formlessness it is, of course, impossible both to produce and reproduce.

The scanty accounts of mental experiences that can be obtained from people unused to introspection, require to be made up by some other means. In our case it seemed most natural to have recourse to drawings made by the observers. Thus they were instructed immediately after every exposure to sketch, as exactly as possible, the figure seen, and, after that, to describe their experiences. All the observers were warned beforehand that some of the figures might resemble or represent something, others not. Sometimes they were asked which had been the case. In order to avoid influencing the drawings by suggestion, this question was always asked after the drawings were finished. One or two initial exposures were given for practice. They were of figures belonging to the B- or C-group that did not resemble any object. The number of observers was 55, 37 schoolboys from 8 to 13 years of age, and 18 adults, chiefly students. The total number of exposures amounted to 527. Of the 45 figures some twenty were shown comparatively regularly. The others were shown to casual persons who were allowed from time to time to take part in the experiments. The experiments were begun towards the end of the spring term 1920 and finished in the autumn of the same year. I here take the opportunity of expressing my thanks to all those who have helped me in my work and to my observers. I also wish to acknowledge my obligation to Prof. Chr. Sibelius, whose tachistoscope I have used, and to Prof. Tigerstedt for permission to work at the Physiological Institute of the Helsingfors University. Further my thanks are due to the lecturer in psychology, Dr E. Kaila, for the untiring interest he has always shown in my work and for the guidance and advice he has given me; and last but not least to Dr C. S. Myers, of Cambridge, who has carefully read my manuscript and suggested numerous valuable corrections and improvements.

III.

We will first consider some of the A-figures. Of these only a small number were used, some unambiguous like 2 A, others ambiguous like 1 A. The unambiguous figures, some of which represent familiar geometrical forms, others animals, and 2 A a human face, were not very

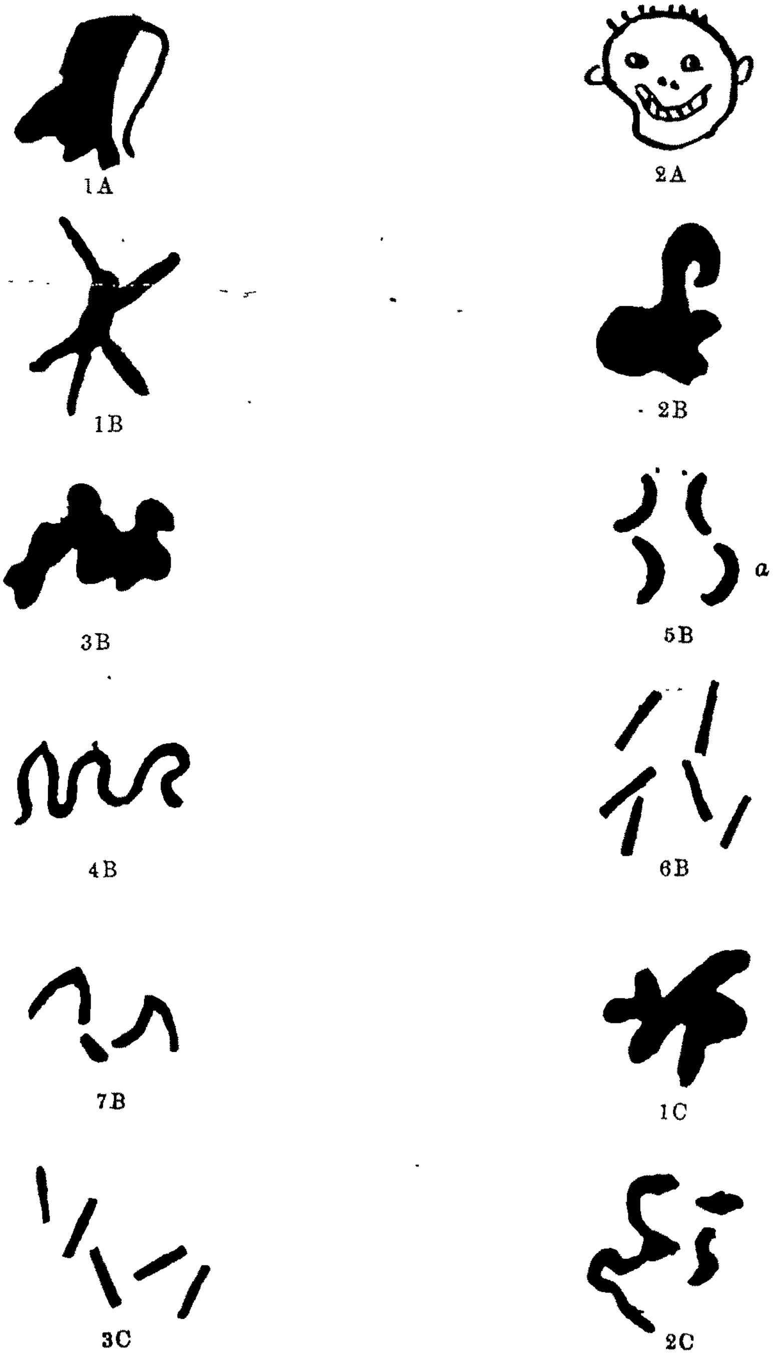

Fig. 1.

Children of 12–13.

Obs. B.C.m. Exp. I. "I could see there was a tail." Exp. II. "Oh, what a lump. And the tail stuck up."

Obs. H.R. Exp. I. The observer was asked if there was resemblance to any object. Answer: "No."

FIG. 4 B. *Children of* 8–9.

Obs. A.W.h. Exp. I. "It's a snake that's crawling."

Obs. P.F. Exp. I. "I did not see quite well. Exp. II. on being asked; "It looked like a snake."

Children of 10–11.

Obs. G.N.r. Exp. I. [illegible]oked like an 'm.'"

Obs. P.N. Exp. I. Draws a rhythmical line.

FIG. 6 B. The replies do not indicate any associations by similarity.

In the above we notice at once the predominance of associations by similarity. This is seen chiefly in the figures of surface, but associations by similarity occur in the linear figures also, if there is any suggestion in them. We seem then compelled to reckon with a suggestive factor—at least so far as children of 8–9 years old are concerned. It was difficult for them to understand the meaning of the word 'figure'; according to their idea the chief thing was to 'look for pictures.' Yet in my instructions I tried to make it clear that the figures did not necessarily represent anything, that some of them consisted only of lines and strokes. The figures were also shown to the children without the tachistoscope after they had made their drawings. Thus I tried to emphasise what had been said. In this way a possible suggestive factor, at least for children of 10–13 years old, must have been minimised.

Even if throughout the experiments on children we allow for such a suggestive factor, we are nevertheless obliged to admit that associations by similarity play a very important part in the children's constructions. They try to find similarities with visual images of a concrete nature. When they fail to do so, they cannot understand the figure (cf. Table I). A few simple schemata are exceptions to this rule.

The table cannot, of course, pretend to be absolutely exact. Very frequently the children were unable to state definitely whether they found resemblance or not; in other cases associations by similarity perhaps appeared only after the construction had taken place, thus without influencing it. Nevertheless we have taken into consideration both the children's statements and their drawings, and sometimes also the number of exposures, though this number has not been included in the purely schematic survey that the table is intended to give. The designation 'definite associations by similarity' indicates that the observer has been able to specify what object the figure represented or resembled, and that the drawings have been influenced by these associations. 'Doubtful associations by similarity' indicates that the observer has either been unable to tell precisely what the figure resembled, or that his drawing clearly represents some familiar object though he has not been able to say what it was. 'Perceptions of detail' means that the observer has either perceived some detail only or not apprehended the figure at all. All other statements and drawings are collected under the head 'Visual schemata.'

TABLE I.

	1 A			1 B			2 B			
Age	8–9	10–11	12–13	8–9	10–11	12–13	8–9	10–11	12–13	Total
Definite associations by similarity	3	9	8	4	3	2	5	7	1	42
Doubtful associations by similarity	3	2	0	0	2	1	1	4	0	13
Visual schemata	1	0	3	0	8	7	0	1	8	28
Perceptions of detail	0	2	1	1	0	1	0	1	2	8
Total number of experiments	7	13	12	5	13	11	6	13	11	91

	4 B			5 B			6 B			
Age	8–9	10–11	12–13	8–9	10–11	12–13	8–9	10–11	12–13	Total
Definite associations by similarity	4	6	0	0	0	0	0	0	0	10
Doubtful associations by similarity	0	2	0	0	2	0	0	0	0	4
Visual schemata	1	3	1	4	7	10	4	6	5	41
Perceptions of detail	0	0	0	2	0	1	2	0	1	6
Total number of experiments	5	11	1	6	9	11	6	6	6	61

Even a superficial perusal of this table makes it evident that the number of associations by similarity decreases with advancing age. If we consider 2 B, we find that of 6 children of 8–9 years old 5 used such associations, that of 13 children of 10–11, 7 show definite and 4 doubtful associations. But of the children of 12–13 only one resorted to association, the others to general schemata. What has been said above holds good for the surfaces. As for the linear figures it may be applied to them also, when any hint of association can be detected in them. (See 4 B.) But when an uncommonly simple schema was readily available, as in 5 B and 6 B, it was used. The latter has been considerably simplified before it fitted into the children's schemata. Some children did not apprehend the figure at all. We reproduce here some of the drawings. Associations by similarity occur in the following figures:

1 A.

9a A top-boot with a strip.
9b Not defined with precision.
9c A sitting man.
11d A kangaroo.

2 B.

9a A boot with a strip.
9h A duck on the water.
9k A water-cock.
11l A bird.

1 B.

9a A star.
9e A fly.
10f, 12g } Symmetry.

4 B.

9h A snake.
10r The letter n.
11s The letters rn.

3 B.

9o A revolver.
9p A tree.

This rather arbitrary selection gives a clear idea of the influence of the associations. A 'blot' with a curved projection conceals within it all kinds of possibilities. Thus 2 B is with different observers 'an elephant,' 'a duck' (9h), 'a bird' (11l), 'a boot with a strip' (9a), etc. The descriptions of 1 A vary still more. The observers have seen it as a 'mill with wings,' 'a kangaroo' (11d), 'a sitting man' (9c), 'a blue-bell,' 'a human face,' 'a top-boot with a strip' (9a). This last association clearly shows how accidental may be the circumstances that affect the children's constructions. It occurred first when 1 A was shown as 'a top-boot with a strip.' Then followed 1 B and then a ten minutes' pause. When 2 B was exposed the observer remarked spontaneously: "I saw a boot with a strip" (cf. the extracts from the records). Previous construction has obviously been influential in producing this idea. The difference between the two figures was nevertheless noticed by the observer; he expressly emphasised the distinction that 2 B was a 'boot,' while 1 A had been a 'top-boot.' This example shows that in the case of children the figures can be related to the very first visual image that makes its appearance in their consciousness. In the records we find many other instances where, in a similar way, an earlier construction has influenced a following one. The observer, *e.g.* who called 2 B 'a water-cock' (9k), had conceived 1 B as 'an insect,' drawing it in the same way as 9e, reproduced in Fig. 2, as 'a fly' with many legs. It seems natural to suppose that the legs are essentially the same as the jets of water radiating in all directions from the water-cock. 2 B was exposed after 1 B; it had only two short broad projections on one side. The long upper projection has been specially noted and drawn by the observer; it is probably meant to represent the water-pipe. It frequently happens that in the construction the same schema is used several times for essentially dissimilar figures. It almost seems as if in such cases the associations by similarity were less potent than the schema in question which happens to be active in the consciousness.

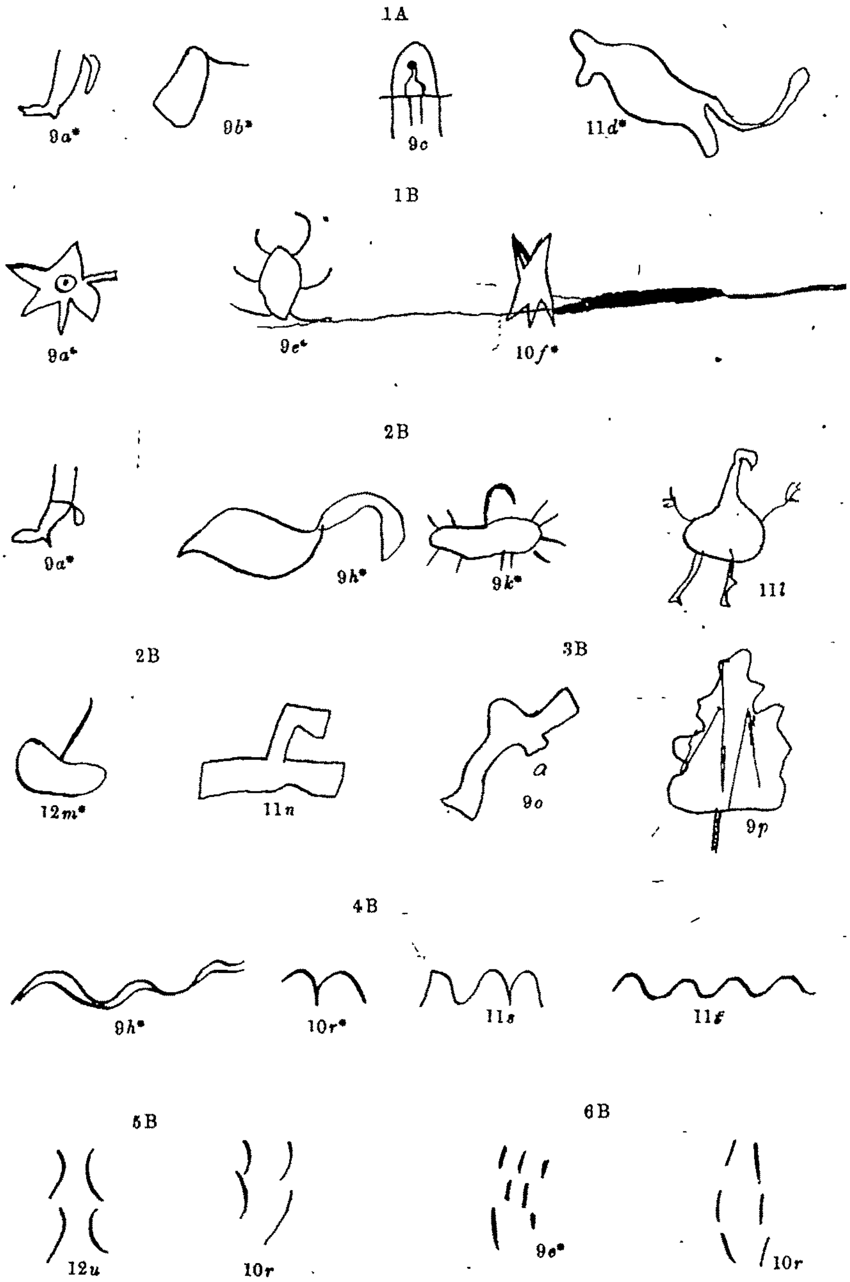

Fig. 2. The letters with * occur in the extracts from the records (pp. 230, 231). The numbers denote age.

'Chance' thus plays a very important part in the children's constructions, rough associations of similarity mediating the transition to images that are for some reason or other easily brought to consciousness.

We have so far used the term 'association by similarity' without specifying what kinds of similarity occur in the children's associations. Yet this must be done if we are to arrive at an accurate idea of the constructions. We must, of course, be able to indicate, if not all, at least some of the points of resemblance between the original sensations and the image created by the associations. If, for this purpose, we compare the figures exposed with the corresponding drawings, we find that similarity, for the most part, has no other source than some simple relation in connexion with a characteristic detail. Taken together, these form a very much simplified schema of the object in question. This fact I will illustrate with some examples. Let us consider 9*h* (Fig. 2), the drawing that represents 'a duck swimming on the water.' The relation here is very simple: A drawn out projection is curved so as to remind one of the neck of a water bird. And if we examine the 'water-cock' (9*k*), we find there the same relation somewhat more closely observed, but the lower projections have, through a casual recollection of the 'fly,' been completely transformed. In the same way we may in all the other drawings belonging to the same figure detect the same simple relation *plus* some detail; and we can be scarcely mistaken, if we say that the varying constructions are due to the fact that the associations are founded on a simple labile perception of relations which is easily adopted in regard to the various images at the moment readily brought into consciousness. The children's ideas of form depend on observations that are made without difficulty. It must, of course, at first, be rather difficult for them to observe and to remember the curvings and transitions of the lines. On the other hand the simple relations between the parts of the whole are comparatively stable and rapidly noted. When the child sees 'a blot' with 'the tail sticking up,' it also easily finds associations by similarity.

All this tallies perfectly with current opinions on the child as artist. According to Meumann[1] an early stage is characterised by the child not really drawing, but symbolising by means of a schema what it *knows* and not what it *sees* (p. 364). It interprets its experiences, not its sensations (cf. 11*l*). The next stage represents the first manifestations of 'the sense of lines and of form.' But strong reminiscences from the preceding stage still remain so that schematic indications are found in addition to the exact representations of the thing seen[1]. The schematic feature seems to be due to the predominance of the simple relations. A child's drawing of a man shows a small 'blob' on the top of a larger one, from which project two strokes, one on each side above, and two others below. The face is comparatively elaborate. Characteristic details are seldom lacking; buttons and spread fingers often recur.

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[1] E. Meumann, *Experimentelle Pädagogik*, Leipzig, 1907.

We have so far principally occupied ourselves with these two stages[2]. Something further may be said of the linear figures. 4B, 5B, and 7B (Fig. 1) are all composed of curves but none of them is quite regular. The last curve in 4B, the line *a* in 5B, and the concave line in 7B break the harmony of the arrangement. For the sake of comparison we are obliged to anticipate things and to emphasise the fact that, with adults, the irregularities are smoothed out and the figures made regular in accordance with the form suggested by them. Children behave somewhat differently. In 4B associations by similarity predominate (cf. Table I). The figures are not conceived as compositions of lines but as 'letters' or as 'a snake' (4B) or 'a flying bird' (7B). This is interesting because in such conceptions the development of our general ideas of form is seen more clearly than usual. We find that many children have the associations 'snake' and 'bird,' where others have pure perceptions of form, *e.g.* of two rhythmic lines. Where 4B is called 'letters,' there is apparently no perception of form. 5B is generally formed according to some schema or other, yet seldom along the lines of least resistance (12*u*), but more generally according to a simpler schema still, four lines curved in the same direction (10*r*); sometimes the children draw only two or four curved lines without plan. Often the figure is not understood at all (cf. Table I). 6B is, as a rule, regarded as an instance of a very simple schema. The most usual form is given in Fig. 2.

In the preceding exposition of our subject we have maintained a distinction between construction according to some schema and construction according to associations by similarity. Let us see how far this is justifiable. The continuous transition from such associations to visual schemata, as suggested by the materials collected in the table, forces upon us the supposition that we have here to do with a difference of degree and development, not of kind; the mechanism to which construction is due in adults, is, in children not fully developed. But its general mode of function is the same. By means of associations the child tries to retain certain simple relations that are familiar to it through earlier experiences. As the experiments show, these relations mostly give rise to images of a concrete nature. That the construction has not as yet become completely mechanised, is shown by the variety of the associations.

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[1] E. Meumann, *Experimentelle Pädagogik*, Leipzig, 1907, p. 365.

[2] It is difficult to draw any exact line of distinction, but it generally seems justifiable to place children of 12–13 on a level with adults—at all events this is so with children of 13.

As will appear later, schematic construction is also founded on similarity of relations. These are characterised by a greater uniformity. Different observers construct much in the same way and we are, therefore, able to detect certain principles of construction, common to all the schemata.

IV.

We now turn to the construction in adults. For the sake of clearness, we will first consider the linear figures. Below are reproduced three different formations of 6B, illustrating three different principles of construction. Two are contained in the first drawing. The adult observer (i) gathers the scattered lines together into groups, and (ii) these groups are placed symmetrically in relation to an imaginary axis. The other two drawings show (iii) a grouping along single lines, the simplest schema conceivable, which is to be found in many different forms. The adult constructs figures 4B and 5B in regular formation according to principles we have already observed in certain children (*e.g.* 11*t* and 12*u*). In the case of 12*u* the figure is symmetrically divided into two congruent parts. In the case of 11*t* a number of similar parts is arranged in a continuous series, roughly symmetrically, two curves on each side of an imaginary central axis; although the figure actually exposed consisted only of three quite irregularly arranged curves. Three regular curves would, of course, also give symmetry, but in most cases the observer gives four. This seems to be due to the 'rhythm' of the

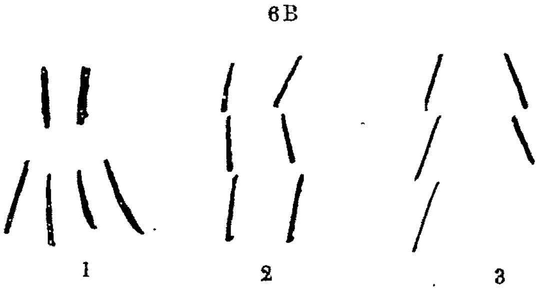

Fig. 3.

line. It suggests rhythmical motion, and where this impression is strong enough, the number of curves is found to be increased. Such expression of a feeling of rhythm is generally independent of consciousness. One observer however said, of his own accord, after a series of exposures, that he had often been uncertain as to the form of the figures, but had then taken refuge in rhythm in forming them. In this exceptional case rhythm was deliberately employed. Another observer also, in 5 B, paid attention only to the curvings of the lines and failed to notice their number, and so—after some hesitation—drew three long rhythmically curved lines instead of four bows. The same observer committed a similar error when drawing another figure not reproduced here. The figure 7 B, too, caused impressions of rhythm.

A few words on the nature of these impressions are desirable. For, as is well known, visual rhythm is a much debated phenomenon. M. K. Smith[1] (pp. 270–88), together with Meumann, has investigated visual rhythm, and their experiments indicate that it depends on motor phenomena. G. E. Müller[2] treats rhythm at some length (pp. 358–85), and concludes that there is no need to regard the motor theory of subjective rhythm as insufficient (p. 367). The motor elements of rhythm are often difficult to discover, as they may be based even on such phenomena as innervations of the larynx.

The impressions of rhythm which the lines made on our observers, I feel inclined to designate as forms of 'synaesthesia.' "Taste and odour of a picture of a fruit, sensations of cold, hard marble produced by the sight of a statue," Külpe[3] gives as examples of synaesthesia. It is true that we do not yet know much about these illusions, but the impressions of linear rhythm show an unmistakable affinity to them. A similar view seems to be held by H. Lundholm[4]. He distinguishes between marginal and motional functions in lines. The latter are due to a suggestive impression of motion, the importance of which is clearly shown by his definition of rhythm. "A rhythmical system of lines is one in which we perceive uniform motion, an arhythmical system of lines is one from which we cannot receive such an impression" (p. 57). The general aesthetic definition is "periodical repetition of an identity." Here then we have a general formula for all rhythm (directly inferred from its nature as a *temporal* phenomenon in the auditory and motor regions of mind) transferred to the visual region, where *space* predominates. It seems to me that the two definitions supplement one another. Lundholm one-sidedly lays stress upon the illusionary aspect of visual rhythm. Certain perceptions of lines are accompanied by impressions of rhythmic motion in the same way as perceptions of marble are accompanied by sensations of cold and hardness. But uniform motion cannot always be identified with rhythm. As a psychological and aesthetic term the word has a fixed meaning, which is comprised in the second definition, and must not needlessly be corrupted. Besides, Lundholm admits that "arhythmic systems of lines can, on repetition, become rhythmic" (p. 214).

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[1] M. K. Smith, "Rhythmus und Arbeit," *Philos. Stud.* 1900, XVI.

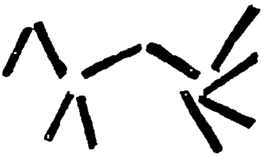
[2] G. E. Müller, "Zur Analyse der Gedächtnistätigkeit u. des Vorstellungsverlaufes I." *Ztschr. f. Psychol.* Ergänz. Bd. v.

[3] O. Külpe, *Vorlesungen über Psychologie* (Herausgegeben von K. Bühler). Leipzig, 1920.

[4] H. Lundholm, *Om objektiva faktorer i konsten*, Lund, 1919.

Jodl[1] thinks the unification of a multiplicity a fundamental law of the aesthetically effective; he distinguishes between eurhythmia and proportionality (p. 127), as its basic forms in the imitative arts. Eurhythmia comprises rhythm, symmetry, and culmination; proportionality includes particularly the law of the *sectio aurea*. Generally, proportionality prevails between the different parts of a whole if none of these parts asserts itself quantitatively at the expense of the others.

The materials already demonstrated essentially confirm this view of Jodl. Even primary forms of construction show a tendency towards unification. The lines are gathered into groups or lengthened into units placed in rows or symmetrically. Very often the symmetry is incomplete or only indicated. We do not find congruous parts on each side of a real or imaginary axis, but accumulations of lines round a centre. An example is given (Fig. 4), in which the upper figure is the exposed original and the lower the observer's drawing of it.



B-fig.

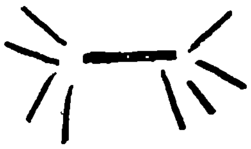


Fig. 4.

The row-formations show both simple and more complicated forms, complication being due to the more or less complete likeness between the elements forming the row. It culminates in impressions of rhythmic motion.

In the linear figures the laws of construction are expressed in the clearest way. Conscious associations by similarity do not occur so readily as in the case of figures of surfaces. But that they may occur is shown by the children's drawings of 4B and 7B. In other respects the children's drawings of the linear figures differ from those of adults chiefly through

[1] Fr. Jodl, *Ästhetik der bildenden Künste*, Stuttgart u. Berlin, 1917.

their greater independence of the laws of eurhythmia. Strict symmetry is comparatively uncommon; groups or whole lines are the commonest schemata. A complete absence of method in the designs is not uncommon in children, but it is rare in adults. As for surfaces, the more developed 'sense of lines' in adults shows itself chiefly in more exact reproductions. The power of observation in adults is better developed, and the capacity for concentration is greater. What has been said above of linear figures holds good, in the main; for surfaces also; construction shows itself in grouping around axes, in symmetrical arrangements, in divisions into congruous or similar parts; in short, in a general tendency towards unification. Where construction or assimilation does not take place, the figure is forgotten or not understood[1].

It is difficult to decide, on the basis of these investigations, to what extent proportionality asserts itself in the construction. The drawings were too simple. Even from a general point of view it is difficult to judge whether proportionality is present or not. Its rules cannot be so clearly expressed as, *e.g.*, those of symmetry. But the fact that proportionality works side by side with the other laws of composition makes it probable that in the construction of the primary impressions it is also involved. The two-fold validity of the laws of form as rules for aesthetic composition and transforming principles has been shown by the comparison just drawn with some of these laws, and can hardly be seriously doubted. It might be added that Bühler has given fairly conclusive evidence of the existence of a direct impression of proportion.

V.

Construction has been studied in our experiments with regard to the transforming influence it exercises on our primary impressions. A survey of the results we have arrived at can be divided into two parts. In the first place we will consider the contents of the general principles of construction recurring in different schemata, and secondly we will describe the formative process in terms of mental function.

I. We have found that construction can be defined, quite generally, as comprehension of new impressions of form, which, by means of associations by similarity, assimilate earlier mental experiences. Further we have found that the associations are chiefly concerned with relational resemblances. This we were able to observe already in children. As has been mentioned above it is naturally impossible for them to observe and to recollect the confused intricacies of the lines. If we have a mobile object, *e.g.* a man, the child can see its contour from different sides, and the lines become difficult to fix. The same holds good, though in a smaller degree, for immobile objects, the child itself being always more or less mobile. The relations between the parts of the objects, on the contrary, are comparatively unchangeable. Straight or approximately straight lines may be compared with these relations as regards constancy. The simple relations and the straight lines thus form a frame that recurs in almost every visual perception, and our experience soon teaches us to use them when we try to comprehend complex forms. The simple relations are in the main, of three different kinds: relations of resemblance, of direction, and of magnitude.

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[1] W. Poppelreuter, *Die psychischen Schädigungen durch Kopfschuss im Kriege 1914–16*, I. Leipzig, 1917. Also above (p. 231).

The impression of proportion, so carefully described by Bühler[1], belongs to the latter group. It is extraordinarily direct, but where great exactness has been demanded, Bühler's observers, when comparing the proportions of rectangles, have had recourse to the relation between the vertical and the horizontal side of the same corner (p. 178). Bühler himself does not take sufficient notice of this circumstance[2]. We are compelled to compare the proportions of the vertical and horizontal sides of rectangles almost at every step. It would be curious, indeed, if this did not take place with a certain directness. Besides, Bühler seems to overlook the fact that all perceptions of magnitude are not exactly similar. We must distinguish between (i) the perception of extent, which is due to factors such as the size of the retinal image and the idea of the greater proximity of the object and is of purely integrative nature, and (ii) the perception of relative magnitude, which is founded on a comparison of two objects. The impression of proportion belongs to the latter category. The fact, that we have an immediate impression of 'bigness' or 'slenderness,' must not induce us to regard it as belonging to the former class. This perception of extent, which, in accordance with Watt's terminology we will, at least provisionally, call 'integration,' together with the likewise immediate perceptions of direction and contour, is a *conditio sine quâ non* for all perceptions of form. But the construction depends on perceptions of relations. The C-figures proved very puzzling to our observers; generally they were not understood. Yet the figures have both direction, extension and outline. But the relations between their parts are not simple enough. The assimilation with earlier experiences, the construction, cannot find connexion with simple relations.

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[1] *Op. cit.*

[2] V. Benussi, "Die Gestaltwahrnehmungen," *Ztschr. f. Psychol.* 1914, LXIX., points out that Bühlor's work is imperfect in so far as he does not explain the position of the relations in his investigation.

The most frequent relation of direction is that of horizontality and verticality. A psychophysical investigation of the same kind as the one Bühler has undertaken for the measurement of the impression of proportion would be interesting with regard to the impressions of horizontality and verticality also. The differential limen would probably show that our judgments of these relations are very exact. For the rest the same that has been said above of the impressions of magnitude holds good of the impressions of direction also; we must discriminate between absolute integrative and relative perceptions of direction.

Resemblance makes itself felt as a tendency to divide the complex into congruous or similar parts. (Cf. the construction into rows and symmetry.)

It is only natural that the straight line, being the simplest exponent of the integrative perceptions of extent, direction, and contour taken together, should play such a fundamental part in the formative process. The simple relations of magnitude, direction, and resemblance are also expressed in the clearest way by means of straight lines.

After this theoretical discussion of the perceptions of relation, we can return to the genetics of construction. We noted that the children did not so regularly as adults construct the linear figures according to the principles suggested by them. The process of construction is not, as yet, quite mechanised; the relations that offer themselves are accidental, founded on such earlier perceptions of form as, with a superficial relational resemblance to the new impression, unite strong reproductive tendencies. The numerous cases where the same schema recurs several times with new details added, are illuminative. Yet it seems more proper in these cases to speak of perseverative tendencies of the schemata in question. And in order to apply this schema in the usual way through associations by similarity, nothing is required but that the relations between the parts of the figure exposed should be approximately of the same kind. Associations by similarity seldom depend on a single detail only. In adults such a form of construction is particularly uncommon. This seems to depend, in some way, on the regularity just pointed out. In the case of surfaces the possibilities of finding associations by similarity are greater. If we compare such different associations with one another, we find that those belonging to the same figure are comparable only in so far as we keep to the relations. Of these the children judge fairly accurately if the relations are simple enough.

Although the construction in adults is, as we have seen, more dependent on the laws of eurhythmia, there is no reason to believe that the continuity of development has anywhere been interrupted and the construction essentially changed. For eurhythmia is nothing but a regulation of relations. Experience seems to have shown that some relations are more common than others. They have stronger tendencies to reproduction, and, if the relations between the parts of the new complex resemble them, an assimilation or transformation takes place. (Symmetry *e.g.* we see wherever we may happen to turn our eyes.) But the new perception need not of course, necessarily, show a complete formal congruity with an earlier one. In the former certain simple familiar relations are fixed which are corrected by the image. Thus arises the peculiar transformation, assimilation, or synthesis found in the construction. The simple relations, which we might with Meinong[1] call 'principal relations,' are the cause of the regularity of the construction. They are subjected to the laws of eurhythmia. The general law of construction might be briefly formulated thus; Construction consists in finding out, by means of associations of similarity, the relations between the parts of a complex.

In my opinion this law holds good in ordinary as well as in tachistoscopic conditions.

One circumstance of extreme importance for the psychology of relations and complexes remains to be touched upon. Can we consider the relational similarities to be direct and independent causes of reproduction, or do they act only in so far as they are contained in the form as a totality? This question makes it necessary for us to adopt a definite attitude with regard to the problem of the 'forms of synthesis.'

The characteristics of the 'form of synthesis' have been described in the clearest way by Gelb[2] and by Höfler[3]. Gelb identifies 'form of synthesis' with the sum of relations. Why introduce a new quality (*Gestaltqualität*) when it can be explained by means of phenomena already known? Höfler does not deny the importance of the relations but holds that the elements of the complex *plus* the relations do not represent a simple addition but a synthesis, the result of which is the form of combination. If I have rightly interpreted Meinong's strange terminology[4], he seems to be of much the same opinion. As Bühler remarks, the views of Gelb and Höfler are not absolutely irreconcilable. Meumann's and Albien's and our experiments show that the perception of a figure, or as Meumann expressed it, "the forming of a clear idea of a figure," requires a special process. We have added that this process consists in finding out the relations between the parts of the figure exposed and is of a peculiar nature. I am inclined to think that the result of this process, 'the form,' bears a characteristic synthetic stamp. This view is corroborated by Bühler's investigations into the perception of proportion. Moreover, Rubin's[1] careful study concerning the difference between the perception of a surface when the background is conceived as forming the figure, and when the figure itself is present to our consciousness, shows that the sum of relations composing a complex produces so peculiar a perception that a special term is wanted for the designation of it. For this we will use the simple word 'form.' The German term *Gestaltqualität* seems to me to have been employed too uncritically, the perceptions of motion, of contour, of extent, of magnitude, of proportion, etc. having, all without discretion, been called *Gestalten*. My own views as to the perceptions of motion and of contour have already been set forth. I have also tried to maintain a distinction between perception of magnitude and of extent, in accordance with my general opinion that 'form' consists in perceptions of relations which must be distinguished from certain integrative perceptions, as for instance those of extent, motion, contour etc. It is possible that some further distinctions will be necessary. Our definition of the integrations does not lay claim to other than provisional validity. It may be added that the integrative perceptions are more constant than the perceptions of form. Under certain conditions the perceptions of motion and contour are always produced, but form may vary with the relations that are fixed in each separate case. Four points arranged in a certain way—this example has become classical in the literature on the subject—may be perceived as a square, a cross, etc.

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[1] A. Meinong, "Ueber Gegenstände höherer Ordnung u.s.w." *Ztschr. f. Psychol.* 1899, XXI.

[2] A. Gelb, "Theoretisches über Gestaltqualitäten," *Ztschr. f. Psychol.* 1911, LVIII.

[3] A. Höfler, "Gestalt und Beziehung-Gestalt und Anschauung." *Ztschr. f. Psychol.* 1911, LX.

[4] Meinong calls contemplation "Fundierung idealer Gegenstände." These "idea objects" are the relations. By "fundierter Gegenstand" he means "form of synthesis."

As has often been emphasised by various writers we cannot observe all the relations at the same time, form being, as we have seen, founded on certain principal relations. By help of these we construct. My view with regard to the problem of form, as it has been developed above, justifies the following answer to the question asked on p. 32. As the perceptions of form must be allowed to possess certain 'complex characteristics,' it is not very likely that those associations of similarity that mediate the construction depend on resemblance of isolated relations; on the contrary the resemblances seem to bear upon the form as a whole where certain principal relations predominate.

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[1] E. Rubin, *Synsoplevede Figurer I.* Copenhagen, 1915.

Seifert[1] has pointed out that the 'need of construction' (*Gestaltungsdrang*) can be a real experience (pp. 70–71), especially, if the construction is in some way suppressed. In fact, the constructive process seems to take place with the automatism of a well-regulated mechanism; in case of any obstruction to its free course, our consciousness is immediately alarmed. Dr Kaila, as observer, remarked: "I almost feel how I construct. It is an extraordinarily concentrated intellectual process, an 'ideational coordination' that takes place. I cannot find any trace of volition."

We have shown that construction, from being a process in which a general law of association was clearly traceable, has become more and more stereotyped and has developed into a mechanism. This mechanism is governed by the same law, but its application has become so schematic that it admits of being limited and fixed by the formulae of eurhythmia. We are scarcely mistaken if from a greater regularity, psycho-physically we infer a greater mechanisation, psycho-physiologically. There are parallel phenomena in the motor regions of mind. There we know of a number of mechanisms that have been created in the course of individual development. These motor coordinations are conveniently adapted to certain stimuli. Both as regards purpose and development the process of construction seems to furnish analogies with them. Following E. Kaila[2], I will term it 'ideational coordination' (p. 23) in which is emphasised the functional resemblance to corresponding motor phenomena.

Similar views seem to be held by Poppelreuter[3]. In opposition to the theory of primary centres of sensation and secondary centres of perception he assumes the existence of "a great number of specific complicated mechanisms of perception" (p. 77)[4]. Throughout the whole of Thorndike's[5] psychology a corresponding line of treatment is to be found.

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[1] F. Seifert, "Zur Psychologie der Abstraktion u. Gestaltauffassung." *Ztschr. f. Psychol.* 1917, LXXVIII.

[2] E. Kaila, *Ueber ideatorische Koordinationen*, Helsingfors, 1917.

[3] W. Poppelreuter, *Die psychischen Schädingungen durch Kopfschuss im Kriege 1914–16*, I, Leipzig, 1917.

[4] He has also investigated such disturbances as may occur in this mechanism and arrives at the conclusion that the most usual abnormity consists in "a false reproduction of the relations of symmetry" (p. 134). This is in perfect harmony with our views as to the nature of Form.

[5] Edw. L. Thorndike, *Educational Psychology*, New York, 1915.

Man's habits and abilities as well as skill, character and temperament depend on original or acquired groups of connexion, *i.e.* mental functions, responding to situations or to elements of situations. A 'situation-response' of this kind is what we seem to have in the process of construction; we understand through schematisation.

As for the construction itself, it can also, by introspection, be distinguished from the primary impression. Thus Dr Kaila, on two separate occasions, was able to recognise a distinct interval between the sensation and the following construction. Very often the observers are conscious of the fact that their drawings differ considerably from the primary impression, which they say they have forgotten. But the contrary is not uncommon. The impression received may be so rapidly formed that the observers are certain that their drawings correspond essentially to the given stimuli, although, on comparison, great differences have been found. The interval between the sensation and the construction seems to have a correspondence in different localisation. This supposition is confirmed by some experiments concerning recognition.

For this purpose a set of figures was used which was comprised of drawings that showed a certain schematic resemblance to one another, although they were made to differ in details. The observers often said that they had seen the same figure two or more times in succession. Recognition is thus dependent on identification not of two sensations but of two schemata, *i.e.* constructed impressions. In the same way we must account for the fact that very characteristic figures are recognised even when turned upside down. On the other hand it is not altogether impossible that recognition may be based on identification of two sensations. On one occasion (I myself being the observer), when the same figure was shown for the second time, recognition took place immediately and with such force that parts of the figure which I knew ought to have been there did not appear at all and the drawing I made differed considerably from the previous one of the same figure. That the figures were identical, I felt quite sure, although it was impossible for me to give form to that primary impression which had caused this judgment. One comment which almost all the observers made, was that the primary impression is very soon forgotten. Perhaps also the construction is a kind of unconscious 'situation-response' by which we try to *retain* our impressions through formulating them.

In the same way as the motor coordinations leave room for individual differences, thus causing different persons to respond somewhat differently to the same stimulus, so also the ideational coordinations may vary within certain limits. For they are, as we have seen, suitable adaptations —which implies that they can be adapted to a certain extent.

Some observers are entirely dependent on the given stimulus. They construct little and cautiously and rather leave out unnoted parts than resort to construction. They often require more than one exposure and are still uncertain about the more complicated figures. On the other side there are observers who transform their primary impressions and treat the elements they have noted rather freely. Earlier experiences are used to a great extent. The result may sometimes be rather surprising. To this category belong most children[1] and some adults. Maybe we have here to do with degrees of construction. In the former case there is more observation than construction, in the latter *vice versâ*. The two types can, it seems, be identified with Messmer's objective and subjective types. Katz[2], who has made experiments with figures, has arrived at the same conclusion, but he, in place of the terms 'objective' and 'subjective' wishes to introduce 'peripheral' and 'central.' As a matter of fact, these terms show more adequately the dependence of the former type on the peripheral stimulus, and of the latter on central processes. The two types are exemplified in Fig. 3, 1 representing the central, 3 the peripheral type. In the former the observer has constructed the figure symmetrically, in the latter he has aimed at a more adequate representation of it.

It may prove of practical importance to maintain the distinction between these types. But as our experiments had not, in the first place, this conclusion in view, and as what has been said above is chiefly intended to be a corroboration of Messmer's, Meumann's and Albien's, and especially Katz's observations, it would need verification. But it may be maintained that we have not really to do with two essentially different types but rather with two different kinds of response to a stimulus; the same person may respond both subjectively and objectively. The former supposition however seems to me more acceptable. The instructions given to the observers directed them to make their drawings as exact representations of the figures as possible, and thus one might expect to find the peripheral response predominant. However, as in each observer we have been able to detect a comparatively constant tendency either towards the peripheral or the central response, it seems to me that there is an actual difference of type.

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[1] O. Messmer, "Zur Psychologie des Lesens u.s.w." *Arch. f. d. ges. Psychol.* 1904, II.

[2] D. Katz, "Ueber individuelle Verschiedenheiten bei der Auffassung von Figuren." *Ztschr. f. Psychol.* 1913, LXV.

(*Manuscript received* 23 *March*, 1921.)

A CRITICISM OF WRIGHTSON'S HYPOTHESIS OF AUDITION.

By H. HARTRIDGE.

(From the Cambridge Physiological Laboratory.)

The essential features of Wrightson's hypothesis which distinguish it from other displacement theories of hearing, such as have been advanced by Rutherford, Watt, de Toule and others, is the claim that there are impulse points in any train of sound vibrations, the time intervals between which approximate closely not only to those occurring in the individual tones which together set the air into vibration, but also to their summation and difference tones.

Wrightson claims that it is the recognition of these time relationships by the brain which causes the appreciation of the individual tones in a musical chord; the perception of overtones, harmonics, summation and difference tones being similarly effected.

The existence of these time relationships are postulated by Wrightson on the following evidence:

If three mathematical curves are prepared as shown in Fig. 3 of Wrightson's book one representing the sine curve of a musical note A, the second the sine curve of another note C, and the third the curve obtained by summing A and C mathematically, and if now a pair of callipers be set to the wave length of the note A and be then tried consecutively on each crest, trough, and crossing point of the compound curve, it will be found that the feet of the callipers will fit certain of the distances on the compound curve. For example on Fig. 3 (p. 26)[1] such distances are shown marked A_1–A_1, A_2–A_2, A_3–A_3, etc. In a complete cycle in one case three such distances are indicated, and in another case four. If now the whole process be repeated with either the wave length of C or with that of the summation tone, difference tone or harmonics of A and C, then in each case a number of coincidences will be found between the feet of the callipers, and certain distances between the crests, troughs, and crossing points of the particular compound sound-wave curve under examination. These distances on the curves represent time intervals, and it is Wrightson's suggestion that the brain in recognising these intervals causes us to perceive the different constituent tones which make up a musical chord.

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[1] *The Analytical Mechanism of the Internal Ear.* Macmillan, 1918.

Wrightson now goes on to state (p. 30) that no mathematical exactness is postulated between the time periods in the compound sound-waves and those of its constituents, but that they may be considered sufficiently accurate for the theory "as the ear, in judging of musical intervals does not depend upon absolute accuracy of vibration ratio, a fact well known to practical musicians." This want of exactness is readily detected in the diagrams which Wrightson gives, thus taking the first (Fig. 3) as an example, it will be found that the distance A_2–A_2 is smaller than the wave length of A while A_3–A_3 is larger.

Boring and Titchener[1] have pointed out that taking the above analysis at its face value the following criticism can be advanced against Wrightson's hypothesis:—(*a*) the summation and difference tones are both an octave too low; (*b*) in addition to the pure tone the ear should also hear the octave below it, which the ear does not appear to do. The reader will conclude from this that even if the fact is ignored that the mathematical analysis is not exact, it still further prejudices itself by leading to false deductions. The proof must be now given that it is the analysis itself which is at fault.

Supposing that the compound curve in Fig. 3 be again turned to and that the callipers be set not to the wave length of either A or C, but to a purely arbitrary distance anywhere between these two values, then if this distance be laid off as described above it will probably be found that it is several times repeated in one cycle. Thus I took a definite series of lengths between 2 and 21 millimeters and counted the number of times that each was approximately in coincidence with distances between troughs, crests or crossing points in half a cycle of the compound curve shown in Fig. 3. The results I obtained are shown in the table below.

Arbitrary length	No. of coincidences in ½ cycle	Arbitrary length	No. of coincidences in ½ cycle
2 mm.	1	12 mm.	1
3	3	13	3
4	2	14	3
5	1	15	3
6	3	16	1
7	1	17	3
8	4	18	2
9	1	19	3
10	2	20	1
11	3	21	1

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[1] Boring and Titchener, *Amer. J. of Psychol.* 1920, XXXI, 101–113.

By averaging the number of coincidences in the above table it is found that any of the arbitrary lengths tested were represented on the average 2·1 times per ½ cycle.

The following table shows the number of coincidences which Wrightson found for the wave lengths of the notes.

Note	No. of coincidences in ½ cycle
A	1½
C	3
2 × *A*	3
2 × *C*	1½

The average number of coincidence works out at 2·25. From the coincident distances which Wrightson marks in his diagram I should exclude (*a*) A^3–A^3 as too large and (*b*) the 2nd C^2–C^2 as too large and this would bring the average down from 2·25 to 2 which is very nearly the value that I found, viz. 2·1, for purely arbitrary distances. Since these arbitrary distances are on the average represented with the same frequency as those which are related to the constituent tones in a chord, with their summation and difference tones, it is hard to understand how the organ of hearing can pick out the pertinent distances and exclude all the others which are equally represented.

I next counted the number of coincidences between some arbitrary lengths and the curve compounded of four different musical tones shown in Fig. 7, p. 29 of Wrightson's book, and compared the average value with that obtained for Wrightson's constituent tones:

Arbitrary length	No. of coincidences per ½ cycle
15·1 mm.	3
21·1	3
26·1	2·5
29	2·5
31	2·5
31·5	3
Average	2·75
Wrightson's lengths	
C	3
E	3
G	2
B	1·5
Average	2·4

The above figures show that as before arbitrary lengths are represented at least as often as those which Wrightson regards as essential to his theory. This, at first sight, astonishing result was partly explained by finding that in the compound curve in Fig. 3 the total number of distances in one cycle varying between 1·5 mm. and 44 mm. in length was approximately 400. Assuming that all distances between 1·5 and 44 mm.

are equally represented each distance would vary from the next by approximately ·1 mm. If then we assume an inaccuracy of setting of say ·1 mm., then any arbitrary distance between 1·5 mm. and 44 mm. will be represented 3 times (one exact fit, one ·1 mm. too large and one ·1 mm. too small) on the average.

Thus we see the explanation of Boring and Titchener's remarks. "It is strange," they say, "that the theory, if we take the diagrams at their face value should thus insist on proving itself. Moreover there is something disconcerting about a theory that will come right on any assumption." This mathematical foundation, which Wrightson had laid for his theory, therefore fails since the brain cannot be expected to pick out certain time intervals and construe them as musical notes, when apparently all time intervals between certain limits are equally represented.

A further test of the basis of Wrightson's hypothesis was now made by combining together three sine curves of the wave lengths 12, 16 and 24 cms. and plotting graphically the compound curve thus obtained. From this graph the rough positions of the maximae, minimae and crossing points were ascertained, and using these values as guides their exact positions were found by calculation. The distances between the different crests, troughs and crossing points in the compound curve were now tabulated between the limiting values of 0 and 48 cms. and were found to number 169 in all. Of these 111 fell close together into 23 groups, there being on the average 4·84 lengths in each group, the average divergence of any length from the mean length of its group being under 1 per cent.

These groups of distances were found to have the following mean values in cms.: 3·67, 5·93, 8·89, 11·01, 11·95, 14·23, 21·69, 22·73, 25·26, 26·35, 27·79, 29·33, 30·36, 31·3, 32·66, 33·58, 36·10, 37·10, 39·10, 40·75, 42·00, 43·30 and 45·00. Now the wave lengths of the tones originally combined for the basis of this calculation were 12, 16 and 24 cms., these should have difference tones of 4, 8 and 12 cms. and summation tones of 28, 36 and 40 cms. All these lengths according to Wrightson should be represented by groups of distances in the compound curve. When however the mean group values are compared with these lengths, it will be observed, that two of the lengths only are represented, viz. the 12 cms tone by a group at 11·95 cms., and the 36 cms. summation tone by a group at 36·1 cms.

There are therefore 6 tones not represented as they should be, and of the groups present 21 should not be there and would represent tones inharmonic with those that should be present; these unnecessary tones would be also in nearly every case inharmonic with one another. If then the brain recognises the presence of these groups and interprets them as tones, as Wrightson supposes, nothing but discord could result.

It would appear that this criticism, taken in conjunction with that advanced by Boring and Titchner, removes from Wrightson hypothesis almost all those features which differentiated it from the telephone theory of Rutherford, and it therefore falls under the criticisms advanced against the older theory. The supposed mathematical basis of Wrightson's hypothesis proves to be illusory.

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SOME EXPERIMENTS ON AESTHETICS.

By LYNETTE FEASEY.

(*From the Cambridge Psychological Laboratory.*)

I. *Aim of the Investigation* (p. 253).
II. *Series I:*
  (*a*) *Methods* (p. 254).
  (*b*) *Results: influence of colour and surface; Bullough's 'perceptive types'; the psycho-galvanic response* (pp. 254–258).
III. *Series II:*
  (*a*) *Methods* (p. 258).
  (*b*) *Results: the 'picture' and 'pattern' types; relation to Bullough's types; additional types; influence of colour and form; the psycho-galvanic response; the emotional basis of the aesthetic judgment* (pp. 258–270).
IV. *Summary of results* (pp. 270, 271).

I. AIM OF THE INVESTIGATION[1].

THESE experiments were originally undertaken with a view to discovering whether the basis of the aesthetic judgment could be rightly said to be emotional, by means of a comparison of the aesthetic judgment with the emotional reaction evoked by the object on which that judgment was passed. The psycho-galvanic reflex was used to measure the emotional reaction. The results obtained by this comparison were, however, entirely ambiguous and it was chiefly the incidental results that proved interesting.

Two series of experiments were carried out: in the first, simple rectangles were used; in the second, arrangements of geometrical figures. Twenty subjects took part in the first series, and twelve in the second. All were adults, the majority being women of undergraduate standing at Cambridge.

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[1] The writer wishes to express her indebtedness to Mr F. C. Bartlett who not only suggested the problem but also gave constant help and advice throughout.

II. SERIES I.

(*a*) *Methods.*

The materials used throughout this series were five rectangles of the following sizes (the size of the base being given first): rectangle 1, 4″ × 3″; rectangle 2, 3″ × 3″; rectangle 3, $2\frac{1}{2}$″ × 3″; rectangle 4, $1\frac{15}{16}$″ × 3″; and rectangle 5, 1″ × 3″.

These five rectangles were used in each of six experiments. In Exps. 1, 2 and 3 the *outlines* of the rectangles only were used. In Exps. 4, 5 and 6 rectangular *surfaces* were used. In Exps. 1 and 4 the rectangles were all white. In Exps. 2 and 5 the rectangles were coloured but were all of the same colour, a pale green. In Exps. 3 and 6 the rectangles were of different colours: (i) dark brick red, (ii) very dark grey, (iii) unsaturated blue, (iv) orange, (v) pale green. Thus Exps. 4, 5 and 6 repeated Exps. 1, 2 and 3 respectively, except that rectangular surfaces were used instead of the outlines only.

Two methods were used in each experiment. In method I, the subject was given the five rectangles and she arranged them in order of preference; in method II, the rectangles were exhibited one at a time and the subject was asked to say whether she liked it or not; the emotional reaction to each rectangle being measured by means of the psychogalvanic reflex and compared with the judgment on the given rectangle.

In order to avoid a mere repetition by the subject of the judgments passed, each subject was not asked to carry out all the experiments by both methods. A subject who performed Exps. 1, 3 and 5 by method I, performed Exps. 2, 4, and 6 by method II.

(*b*) *Results.*

Subjects naturally differed widely in their preferences, as also in the decidedness of their preferences. One or two were unable to state any preferences at all, and others experienced considerable difficulty in doing so, particularly when the rectangles were uncoloured. Others had no hesitation whatever. The general order of preference may be found by totalling for each rectangle the numbers which give the position of that rectangle for each subject. Then, since 1 means that the rectangle is placed first, the rectangle with the lowest total will take the first place in the general order of preference. The order of the rectangles is found to be: 1, 4, 2, 5, 3, *i.e.* the rectangle 4″ × 3″ is liked best, and the rectangle $2\frac{1}{2}$″ × 3″ least.

This order is the order for all the six experiments. If, however, we consider the first three experiments alone we have the order of preference for the rectangular outlines, and can compare this with the order for the rectangular surfaces, as found from the last three experiments. These two orders are:

for the outlines:—4, 1, 2, 5, 3;
for the surfaces:—1, 4, 2, 3, 5.

Thus though there is a difference in the order according as the outline or the whole surface is given, this difference is not a very marked one: rectangles 1 and 4 are in both cases preferred to rectangle 2, and rectangles 1, 4 and 2 to rectangles 3 and 5. The fact, however, that there *is* a difference, is supported by the subjects' introspections. When the outlines were replaced by the surfaces, one subject, for example, said "They give a totally different impression now."

We find a very much more strongly marked difference between the order of preference when the rectangles are coloured and that when they are all white, or all of the same colour.

Exps. 1 and 4 (all white)	give	the order:—	1, 4, 2, 5, 3.
Exps. 2 and 5 (all same colour)	,,	,,	1, 4, 2, 3, 5.
Exps. 3 and 6 (different colours)	,,	,,	1 and 5 (equal) 3, 4, 2.

In Exps. 3 and 6 rectangle 2, which was placed third in Exps. 1 and 4, takes the lowest place, owing to its dark grey colour, and rectangle 5 is liked best because its colour is liked. These results seem to indicate a much greater interest in colour than in shape in these experiments: the combination of colour with the shapes being able completely to upset the order of preference assigned to the shapes alone. This greater interest in the colours than in the shapes is also shown in the introspections. Many of the subjects remarked that they were much more interested in the colours than in the shapes; and one said "You do not think of the shapes when the colours are there." By many of the subjects the reasons given for the order assigned to the rectangles in Exps. 3 and 6 refer almost entirely to the colours: and the coloured rectangles appear to be treated simply as specimens of certain colours. There are, of course, exceptions. Some subjects really do consider both colour and form in fixing the order of preference, and make such remarks as that "dark colours are better for big things." One or two allow their preferences for the shapes to outweigh their colour-preferences, but these are decidedly in the minority.

Rectangle 4 has, approximately, the proportions of the Golden Section rectangle. It is of interest, therefore, to notice that it takes a high place, though not the highest, in the order of preference, except in Exps. 3 and 6 where the shape was not liked sufficiently well to outweigh the dislike of its orange colour. If this rectangle had been placed 'the other way on,' so that the breadth was greater than the length, it is quite possible that it would have taken a still higher place. Indeed, several of the subjects remarked that they would have liked it better so placed. If this is so, the popularity of the Golden Section rectangle cannot be due to the mere physical proportions of the lines.

The remarks made by the subjects during the experiments, and the reasons given by them for their preferences, show that the four 'perceptive types,' which Mr Bullough found in his experiments on the aesthetic appreciation of colours and colour combinations[1], and Dr Myers in his experiments on listening to tones[2], appear also in the aesthetic appreciation of simple shapes such as rectangles. Examples of the four types are as follows (a subject may be said to belong to a certain type when she gives a preponderance of judgments of that type):

(i) *Objective.* It seems possible to make three subdivisions of this class—

(*a*) The subject may merely criticize the proportions, the rectangle being described as neat, compressed, straight, too long and narrow, regular and so on.

(*b*) The rectangles were frequently judged according to their usefulness for certain purposes, for example, for picture frames. Then we have such judgments as that the rectangle is "quite useless" or "the best shape for a photograph."

(*c*) Mr Bullough points out[3] that appreciations of the objective type "appear to be special cases, of a large class of appreciations based upon what might be called 'conformity to type.'" The object is judged by some preconceived "type or standard which the subject has set up for this class of thing." Certain judgments occur which seem to be explicitly based on this principle: such judgments, for example, as "it looks more like a rectangle," "it is definitely a rectangle," "it is a convenient orthodox shape."

(ii) *Subjective.* Here the subject's attention is directed towards her self and her own feelings rather than towards the object, the rectangle.

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[1] This *Journal*, 1908, II, 406–463.

[2] This *Journal*, 1914, VII, 68–111.

[3] *Loc cit.* 451.

(*a*) Sensual: *e.g.* "it is 'trying' to look at," "it gives you a cramped feeling."

(*b*) Affective: *e.g.* the rectangle is characterized as monotonous, undisturbing, too familiar, or as appealing to the imagination.

(iii) *Associative.* The associations which determine the judgments may be either

(*a*) Impersonal, as "it is like doors in a doll's house," "like fortress windows, it suggests the Middle Ages," "too like a Tate sugar box," or

(*b*) Personal, as "it is too geometrical, the kind of thing we always had to work out problems with at school." One subject disliked the square because "I never could draw squares." Another liked a rectangle because it suggested the frame of a particular picture which was valued.

(iv) *Character.* Judgments of this type are rare but a few do occur. This type appears in such judgments as "it looks generous," "it is a generous sort of creature," "it suggests liberalness," "horrid mean little thing." One subject gave as her reason for disliking squares that "squares have no individuality of their own," and added "it is much easier to be regular than to show individuality." The same tendency to treat the rectangles as if they were persons appears in such remarks as "it looks as if it were trying to be a square," "it looks as if it were pretending to be a square."

Most subjects give judgments of more than one type; but, roughly speaking, subjects tend to give judgments of the same type for both colour and shape in these experiments. For example one subject who gave some character judgments on the shapes such as "generous" and "mean," described the colours as "opulent," "fierce," "wild," "downright." Another who described the shapes as neat, compressed, etc., characterized the colours as dark, dull, brilliant, and so on.

The comparison of the emotional reaction with the aesthetic judgment, gave, as was said above, quite ambiguous results. In only four of the twenty subjects was there any suggestion of a correspondence between the amount of the galvanometric deflexion and the degree of aesthetic appreciation. These four subjects did indeed show a rough correspondence. The chief difficulty of the method, assuming that the psycho-galvanic reflex really expresses the amount of the emotional reaction, is the practical impossibility of knowing how much, if any, of the emotion registered is the effect of the given stimulus, and how much should be assigned to simultaneous stimuli. All that we can say, then, is that while in certain subjects there is some evidence of a correlation between the aesthetic judgment and the emotional reaction, in the majority of cases there is no evidence whatever of any such correlation and no conclusion whatever can be drawn as to a possible emotional basis of the aesthetic judgment.

III. SERIES II.

(a) *Methods.*

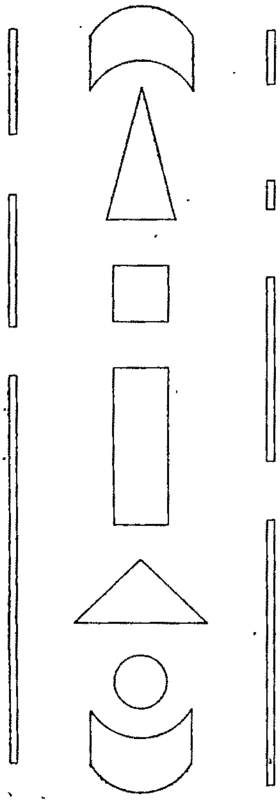
In the second series of experiments a number of geometrical figures, a circle, a square, lines, triangles, etc., were used. The figures were cut out in tin and two sets of them were used, one set of fourteen figures being uncoloured while the other set of fourteen were coloured by means of differently coloured papers stuck on to the tin.

Again two methods were employed. Method I was constructive: each subject was given first one set, then the other set of figures, and asked to arrange them on a piece of grey cardboard 18″ × 12″, in the way which she thought looked best. Thus 24 different arrangements were made, two by each subject. In method II the galvanometer was again used. The arrangements made by all were shown to each subject and the judgment passed on each of the 24 cards was compared with the emotional reaction to the same card. The subject was also asked which of each set of cards, coloured and uncoloured, she liked best. Nothing was said by the experimenter as to the presence of the subject's own two arrangements among the cards, but if the subject herself made no remark, she was asked at the end of the experiment whether she had recognized her own card.

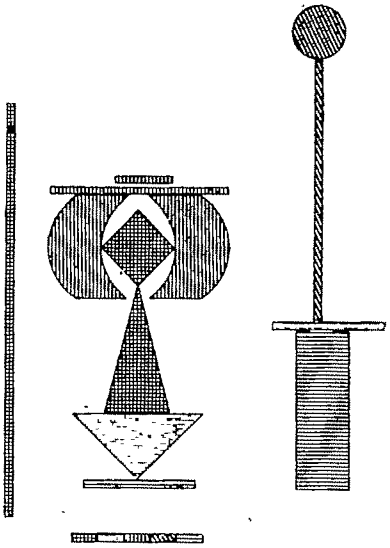
(b) *Results.*

The most interesting result was the demonstration of two quite distinct attitudes on the part of the subjects towards the cards. Some subjects made purely formal geometrical arrangements of the figures, aiming solely at symmetry and balance; while others arranged the figures so as to make a picture of an object or scene, such as a church, a flower, a village scene. The former will be called subjects of the 'pattern-type,' and the latter, subjects of the 'picture-type.' The clearest example of the pattern-type is Subject V, and of the picture-type Subject VI; cards made by these two subjects are reproduced as *a* and *b* on the opposite page.

A merely casual inspection of the cards suffices to show the presence of these two types. Almost all observers, for example, recognize at once the different types to which Subjects V and VI belong. But it is not safe to trust entirely to the cards to show to which type a particular



a



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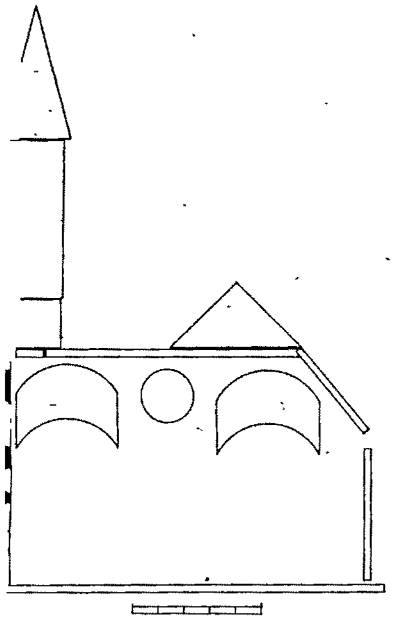

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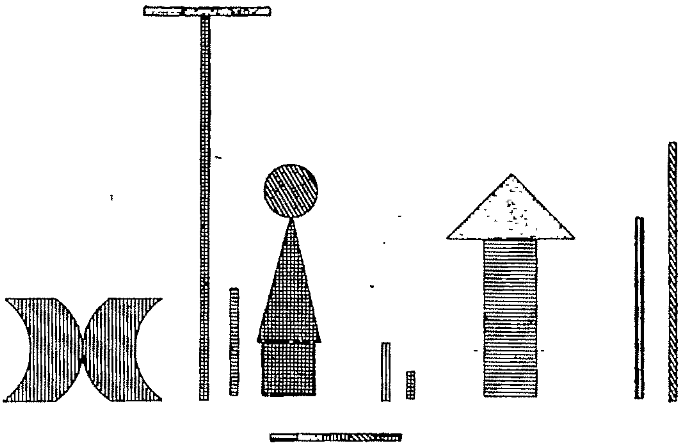

d

subject belongs, as whether any card appears as a 'pattern' or a 'picture' depends partly on the observer's own type. The subjects' introspections provide a more reliable guide. One or two quotations may be given. The following remarks were made, during the construction, by one subject clearly of the pattern-type—"Symmetry, I suppose, is the idea. I will have something rather large in the centre to place the other things round. By way of contrast I will have these two triangles here. I wish the big triangle had been made more symmetrical. I will put a big line here to take away the feeling of its being unsymmetrical. I am trying to get a central axis down the middle." Some subjects of this type did, indeed, aim at some particular design, for example a diamond shape, but this was much less common.

In striking contrast is the following quotation from one of the picture-type subjects: "I am trying to construct a pattern in my mind; I am thinking of it in terms of architecture. These are two domes: it is a piece of Moorish architecture and the lines are the slender columns. I am not thinking of it abstractly as a figure but definitely as some fragment of a building, a gateway or something like that. Yes! it's going to be an absolutely bizarre Oriental gateway. Now it's part of an Indian temple: the figures are mystical signs that had to be worked in in an arbitrary way and this is how they were worked in. It's a cabalistic door."

Some subjects, of course, did not work out the idea in so much detail; *e.g.*, one merely said, "I tried to make the whole look like a flower." As one would expect, the subject's interests, permanent or temporary, often played a large part in determining the object constructed. The subject from whom the above quotation is taken, for example, showed all through the experiments an interest in the East and finally told the experimenter that she was going to India the following year. Another subject, who made a picture of a flower, was interested in botany and so, as she said, "the coloured figures suggested flowers at once."

Of the twelve subjects who took part in this series of experiments, three were definitely of the picture-type, four were equally definitely of the pattern-type, while the other five belonged less definitely to one or other type or were of different types in the construction of the two cards. The two types differed somewhat in their methods of construction. Pattern-type subjects had certain definite principles which guided them in the construction: they were therefore surer of what they wanted to do. Subjects of the picture-type, on the other hand, did not follow such principles and therefore seemed more uncertain how to begin. Usually they made one or two random arrangements of a few of the figures until by chance one such arrangement suggested an idea, after which the construction went on much more confidently. Subject VI said, "I started with no idea: then the top gave the idea of a steeple and then I tried to make it more realistic." When given the coloured figures, she said, "I can't get any inspiration at all for this one." The change from uncertainty and hesitation to confidence and enjoyment when at last the 'inspiration' came was most marked.

The two types are found not only in the construction experiments, but also in the observation of the arrangements made by others. Whether any given card appears to the observer as a pattern or a picture depends on both objective and subjective factors: it is determined partly by the actual positions of the figures, and partly by the observer's own type. Ten of the twelve subjects, including some who were most definitely of the pattern-type, took Subject VI's uncoloured card to represent a church. On the other hand, the fact that one of the subjects did not even see this card as a picture of a church shows the difficulty which a pattern-type subject may find in realising the picture-type attitude. This subject merely said, "The person who made this seems to have had some sort of idea in her head, a factory or something like that." Again, one card which was variously interpreted by subjects of the picture-type as "a silhouette over a garden wall with a tower, some trees and the moon," and as "a village scene, with a sign post and a church," was intended by the subject who made it to be a formal design. One picture-type subject, on being told that this card had not been intended to represent anything at all, exclaimed, "But the black is so obviously a church!"

Two examples may perhaps be given (*c* and *d* on page 259) to illustrate the different judgments passed on the same card by subjects of the two types:

(i) *Card X* (*c*) *Coloured.* Pattern-type. "Yes, that's rather nice. The colours are nicely balanced and the proportions are good. It is nicely placed on the paper."

Picture-type. "I was rather disturbed by that. It is a flower in a pot with a cheeky young flower at the right showing how much taller it is. There are no leaves. The black is a measuring rod."

(ii) *Card XII* (*d*) *Coloured.* Pattern-type. "It is poor. The lump of red knocks you down absolutely and there is nothing whatever to balance at the other end."

Picture-type. "I was awfully tickled by that and made a long story about it. The short lines are children. It is Noah and his children. The red is a huge butterfly out of the Ark. It is most interesting."

Subjects belonging very decidedly to one type often tend to like only those cards which seem to them to conform to their type, and sometimes even to regard that type as the only reasonable one. One picture-type subject condemned all the cards which she could not interpret as pictures as "lacking in imagination." Similarly Subject V, who is markedly of the pattern-type said of Subject VI's uncoloured card, which she saw as a church, "That is ingenious but awfully stupid really." Of the same subject's coloured card she said, "I don't like it as much as the very regular ones, the proper sort of thing." But this is not invariably the case. A subject may see most of the cards as patterns and yet like best the few which she does see as pictures, while another may see most of the cards as pictures and yet she may regard these merely as amusing and like the patterns best. Most of the subjects, however, decided at once that the cards fell into two classes and were prepared to judge each class on its own merits. One subject when asked which class she liked best said, "It is difficult to say; they are so very different."

Generally speaking, a subject who was of one type in construction was of the same type in observation. There were, however, some striking exceptions. Subject VI, for example, was of the pattern-type in observation.

In the first series of experiments it was found that the four perceptive types demonstrated by Mr Bullough in the aesthetic appreciation of simple colours and colour-combination and by Dr Myers in the appreciation of tones appear also when simple shapes such as rectangles are used. With the more complex material used in the second series it is not possible to place all the judgments under one or other of these four heads. It seems necessary to recognize seven classes of judgments.

(i) *Objective judgments.* These are judgments based on such principles as balance, order, regularity, symmetry etc., *e.g.* "nice and symmetrical," "badly balanced," "too spread out," "uneven," "too scattered." In this class are found also judgments depending on the presence or absence of unity; *e.g.* "there is no correlation between the parts," "all the parts fit in together," "nothing has anything to do with anything else," "it is all connected up."

In the objective type, again, are found judgments referring to the usefulness of a design for certain purposes, *e.g.* "that would be a nice design for a jumper," "that would make a jolly border for a nursery."

(ii) *Subjective judgments.*

(*a*) Sensual, *e.g.* "that makes me feel cool," "it gives you an airy feeling because of the spaces," "it makes me feel queer."

(*b*) Affective, *e.g.* "dull," "uninteresting," "it disturbs me," "worrying," "disappointing," "disturbing," "exciting," "amusing."

(iii) *Associative judgments.* As with all kinds of material, associations play some part in determining the aesthetic judgments passed on the cards. Examples are "that's like a goal post, it reminds me of games," "that is a rough sketch of a village scene, it called up a village scene at home," "the top part reminds me of a lantern I once saw at a dance."

It might perhaps be suggested that whenever a subject is of the picture-type the judgments must be held to be determined chiefly by associations, as they are determined by the objects suggested by the shapes. But it seems scarcely possible to regard the fact that subject VI's uncoloured card suggested a church to most of the subjects as a case of association any more than one attributes the fact that a drawing of a church is recognized as such to association. It is often difficult to decide whether a judgment such as "That's awfully jolly. It's just like a pigeon cote," is to be called an associative pattern-type judgment or a true picture-type judgment. Everything depends on whether the card is taken to be really a picture of a pigeon cote, church, or flower, or whether it is really regarded as a formal arrangement of figures which suggests other objects. Examples such as "That's a rough sketch of a village scene: it called up a village scene at home," show that picture-type judgments are not necessarily associative, but may themselves call up associations, just as a drawing of a church may suggest a church one knows. Judgments such as "it reminded me of a weather-cock," appear to represent a transitional stage between the pattern and the picture-types.

(iv) *Character judgments.* Judgments such as "quaint," "it is daft, it looks flippant," "ambitious," "strange," "queer," "prosaic," "ordinary," "stupid," "it hasn't much character," "it has absolutely no character, it is typically stupid," "it is striving after peculiarity," are judgments of the kind we pass on people and therefore represent Mr Bullough's 'character type.' In this class also we must put judgments attributing personality not to the card but to the objects supposed to be represented, *e.g.* "it is a flower in a pot with a cheeky young flower at the right showing how much taller it is."

(v) *Judgments referring to the artist.* Judgments of the character-type are closely related to judgments on the person who made the card. It is not always easy to decide whether the judgment "it is stupid" or "ambitious" really means—"the card is stupid or ambitious," or whether it means—"it was stupid or ambitious to make a design like that." Undoubtedly they are in many cases true character judgments. There are, however, many judgments which explicitly refer to the artist, *e.g.* "they have tried to make it regular but the pieces wouldn't admit of it," "that is a neat way out of the difficulty," "it is careless; the pieces are merely thrown on as if it were done in a hurry." There are frequent commendations of the designs as "cleverly done," "very ingenious," "very original," and many references to success or failure of the artist's aim, *e.g.* "it is a failure," "it is muddled-looking, it hasn't achieved what it set out to do," "it has achieved something, made something quite obvious what it is," etc. Judgments of this class pass over into character judgments when the adjectives are transferred from the artist to the card.

The occurrence of so many of these judgments referring to the artist probably finds an explanation in the conditions of the experiments. Each subject had first to construct two designs herself and was usually very anxious to see those made by the other subjects. Hence when these were shown to her, her attention was liable to be drawn from the card to the person who made it, particularly where the subject was much impressed by the card. Such judgments appear, then, to represent a social factor in aesthetic judgment.

(vi) *Judgments referring to the suggestiveness of the design.* There are a number of judgments which may be classed together, which seem to have affinities with more than one of Mr Bullough's 'types,' *e.g.* "very imaginative," "very suggestive," "it stimulates the imagination," "it makes one think of something else directly," "it suggests all sorts of quaint things," with the corresponding negative judgments, *e.g.* "distinctly lacking in imagination." These are obviously related to the association type: a design is commended because it suggests "a lot of quaint things," etc. Differently expressed they approach either the subjective- or the character type judgments; the judgment "it stimulates the imagination" resembles the subjective judgment as the subject is chiefly concerned with the effect of the card on herself, while the judgment "imaginative" may quite well be a true character judgment.

(vii) *Judgments referring to the presence or absence of meaning.* Lastly, there are a number of judgments referring to the presence or absence (usually the absence) of meaning in the design: *e.g.* "that doesn't mean anything," "it doesn't convey anything," "the bottom part looks so curious stuck down there, it has no meaning at all," "I don't like that; it's meaningless. The others either represent something or are definite designs that could be used for something," "I cannot see any reason in it at all." These judgments are not confined to subjects of the picture-type: they are not condemning a design merely because it does not represent some concrete object. One subject said, "I don't like them where they don't convey anything." When asked whether this meant she only liked the pictures she replied, "Oh no! They may convey something even where they are not pictures. I don't like them when they are untidy." This reference to 'untidiness' suggests that the absence of meaning may underlie judgments of dislike based on the absence of unity, order, balance, etc. The presence of meaning appears to be implied in the appreciation of designs which stimulate the imagination or suggest ideas. Although, somewhat to one's surprise picture-type subjects very rarely make any reference to meaning, positive judgments of this type seem to be implied very frequently: designs which appear to be pictures are usually liked best because they are more readily appreciated as having meaning. Many subjects indeed appeared to think that the fact that the card represented some object was a quite sufficient reason for liking it. Thus these judgments referring to meaning probably do not represent a distinct class of judgment but merely make explicit an essential condition of any object's being liked. Anything that thwarts the 'effort after meaning' is disliked.

Summarising, we may say that the same four types found in the aesthetic appreciation of colours and tones and simple forms occur also in the appreciation of more complex designs, but that all the aesthetic judgments cannot be put under one or other of these four heads. We must recognise three other classes of aesthetic judgments: those referring to the artist, which represent the social factor in aesthetic appreciation and are related to the character type; those referring to the suggestiveness of the design, which have affinities with the associative, subjective and character judgments; and those referring to the presence or absence of meaning which appear merely to make explicit what is implied in all the judgments[1].

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[1] In his paper already referred to, Dr Myers makes some important elaborations of the four perceptive types suggested by Mr Bullough and it is of some interest to consider how far the present results are in agreement with those of Dr Myers. Dr Myers recognises three subdivisions of the *intrasubjective* type (Mr Bullough's physiological or subjective type) viz.

(i) judgments referring to changes in the cognitive aspect of the subject's consciousness, This classification of the aesthetic judgments cuts across the division of the judgments into pattern-type and picture-type: that is, one cannot say that a subject who belongs to the objective type (*i.e.* gives a preponderance of objective judgments) will be necessarily either a pattern-type or picture-type subject. The table given below shows roughly the relations between the two classifications. As it is not always easy to say definitely to which type a subject belongs, the judgments, not the subjects, have been classified; and the table shows what percentage of pattern- and picture-type judgments fall under each of the seven heads given above. A judgment is called a 'picture-type judgment' when it is passed on a design which is regarded as a picture. A judgment condemning a design *merely* because it is a pattern and not a picture is, however, called a negative picture-type judgment.

The most noticeable feature revealed by the table is the fact that while 63 % of the pattern-type judgments are objective, only 11 % of

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(*a*) leading him to describe the tones as strident, velvety, etc.;
(*b*) exciting definite tactual, motor or organic sensations.
(ii) judgments referring to changes in the emotional state, mood or feeling attitude of the subject.

Corresponding to these two subdivisions of the intrasubjective type are the judgments classed above as respectively 'sensual' and 'affective.'

(iii) judgments referring to changes of a conative nature in the subject's consciousness as seen in
(*a*) impulses, *e.g.* to turn the head;
(*b*) the endeavour to determine the meaning or use of a sound.

Very few of judgments of this type were found in the present experiments. One subject, however, said that a certain rectangle made her "want to scream" (cf. (iii) *a*); and judgments of type (iii) *b* seem to be implied in such statements as "I can't make that out at all," "I can't think what the red thing can be."

Dr Myers also recognises under the *objective* type
(i) the appreciation of the object as having meaning or use;
(ii) the consideration of the sound in relation to the subject's standard of purity, pitch, etc., which a satisfactory sound should attain.

This latter type of judgment is definitely included in the objective type in Series I and though not occurring in a definite form in Series II seems to be implied frequently in judgments which are based solely on such principles as regularity, *i.e.* the subject seems to think that a design is satisfactory if it attains a certain standard of regularity.

Judgments based on the appreciation of the object as useful for certain purposes have in the present experiments (as also by Mr Bullough) been included in the objective type. Series II, however, suggests that judgments based on the appreciation of the object as having meaning (using 'meaning' in its widest sense, and not as equivalent to usefulness) are not confined to the objective class and they have therefore been taken to constitute a separate type (type 7). I understand that Dr Myers now regards judgments depending on the appreciation of the object as having meaning or use, not as a subdivision of the objective type, but as a distinct type to which he gives the name of the 'pragmatic' aspect.

the picture-type judgments are objective. The class claiming the largest proportion of the picture-type judgments is the subjective. Judgments referring to the 'suggestiveness' (type 6) we have seen to be closely akin to the subjective- and character type judgments, whereas character type judgments are nearly related to the subjective judgments; indeed Mr Bullough regards them as a further development of subjective judgments. If, then, we may be allowed to group together types 2, 4 and 6, the respective proportions of subjective and objective judgments would be as follows:—

TABLE I.

Type	Pattern	Picture
	%	%
1. Objective	63·1	11·1
2. Subjective	17·1	37·1
3. Associative	1·1	3·7
4. Character	10·2	17·3
5. Referring to Artist	5·3	14·8
6. Referring to Suggestiveness	—	13·6
7 Referring to Meaning	3·2	2·4
	100	100

TABLE II.

Type	Pattern	Picture
	%	%
Objective	63·1	11·1
Subjective	27·3	68·0

If under the term 'subjective' we had included judgments referring to the artist (which usually are closely related to the character judgments), we should have had a still more striking result. These results seem to justify the statement that the pattern-type is pre-eminently an objective type, while the picture-type is pre-eminently of the subjective (or character) type. There are, of course, exceptions: *e.g.* Subject V, quite definitely a pattern-type subject, gave several character judgments, describing the patterns as "stupid," "lacking in individuality," etc. Further, because picture-type subjects tend to give judgments of the subjective or character type, we cannot assume that their judgments are necessarily of higher aesthetic value; the pure subjective judgment appears to be of lower aesthetic value than the objective, while the character judgments are of higher aesthetic value. The pattern- and picture-types are perceptive not aesthetic types, and it is not easy to decide which of the two attitudes is the more nearly related to the true aesthetic attitude.

In the first series of experiments it was found that the subjects had a much greater interest in the colour, than in the form of the rectangles; colour-preferences quite outweigh the preferences for shapes. It is interesting, therefore, to find that with the more complex material used in Series II the relative importance of colour and form is reversed. This is shown both in the construction of the cards, and in the observation of those made by others. In the construction, where a satisfactory arrangement of both colour and form is found impossible, it is the colour scheme that is sacrificed. This is even more markedly so with picture-type subjects than with pattern-type subjects, as is shown by the introspections. Picture-type subjects make very little reference at all to the colours. In the observation of the designs made by others the same feature occurs. An inspection of the remarks passed by all the twelve subjects on the twelve coloured cards reveals only 33 references to colour, many of these being quite incidental remarks, and practically all of them also accompanied by remarks on the shapes. Of these 33 references to colour, only six are picture-type judgments. A card may be taken to represent "a village scene," "a pagoda," "a silhouette over a garden wall" with no reference to the colour scheme and apparently with little or no thought of the improbability of the colours. For picture-type subjects form is even more coercive than for pattern-type subjects.

In Series I the shapes were so simple that they were apt not to be considered at all, the coloured rectangles being regarded merely as specimens of certain colours. With increasing complexity of the material, form appears to become increasingly important and we may conjecture that with still greater complexity, for example in the case of pictures, form would assume still greater importance relatively to colour.

Subjects of the two types show, as we should expect, a somewhat different attitude to colour. Pattern-type subjects aim at balance and harmony of colours and usually effect a compromise between symmetry of form and harmony of colour. They make such remarks as "The black is the background as it clashes least. I tried to help out each with the others by putting black between." Subjects of the picture-type, where they considered the colour scheme at all, aimed not so much at harmony of colours as at a realistic representation of the object. When given the set of coloured figures—in which the circle was green—one of them said, "I shan't be able to make anything out of this, I'm afraid; they've made the sun green." Subject X, for example, in her coloured design, is not so much concerned with the question as to whether black and green go well together as with the fact that the green would do for a plant and the black for its pot. The coerciveness of the form for subjects of this type appears in the fact that even if the colours had been interchanged, *i.e.* if the line had been black and the rectangle green, the line would still have had to represent the plant, and the rectangle the pot.

The use of the two methods in this series of experiments, the method of construction and the method of judgment, made it possible to attempt to discover what influence, if any, the fact of construction has on aesthetic appreciation . Since it appears to be a matter of everyday observation that one tends to be partial to what one has made oneself, we might expect a subject to tend to pass a more favourable judgment on those designs which she has made herself than on the others. It is somewhat surprising to find that only one of the twelve subjects liked her own cards best, though others remarked that they had "a certain affection" for their own, or that "if it had been anyone else's" they should have "detested" it. These remarks show a tendency on the part of some subjects to feel a partiality for their own designs merely because they *are* their own: but this tendency is not sufficiently strong to make them like their own best. On the other hand a subject may not like her own at all; we find such remarks as, "That's mine, I don't think much of it at all," "that's my hideous thing," "mine doesn't compare at all well with the others." All the subjects recognized their own cards, some giving big emotional reactions (as measured by the galvanometer) to them; but there is no clear evidence that the fact of having constructed a particular design necessarily determines a favourable aesthetic judgment on that design, although in some cases it certainly does so, to some extent at least.

In the first series of experiments, as stated above, no evidence was found of a correlation between the aesthetic judgment and the emotional reaction, as measured by the psycho-galvanic reflex, except in four of the twenty subjects. In Series II there was a similar absence of correlation. This absence of correlation is sufficiently well indicated by the fact that in the large majority of cases the deflexion shown by the galvanometer for the card which the subject liked best was *less* than the mean deflexion for the set: in no case was it markedly above, and in many cases it was very markedly below the mean deflexion.

This absence of correlation between the deflexions (representing the emotional reactions) and the aesthetic judgments cannot, however, be taken to *disprove* the emotional basis of the aesthetic judgment. In the first place, the difficulties of the method were such as to render it impossible to conclude that there really is no correlation between the aesthetic judgment on a stimulus and the emotional reaction to that stimulus. In the majority of the subjects it is quite impossible for the experimenter to tell how much, if any, of the reaction is a reaction to the particular stimulus exhibited, and how much is due to simultaneous stimuli, material or mental. Many of the subjects were quite obviously reacting to other stimuli, large deflexions taking place between the exposures of the cards, and not only were these apt to mask the reactions to the given stimuli, but they emphasize the fact that the deflexion which appears to be the reaction to the given stimulus may really be partly, at least, due to simultaneous stimuli quite without the range of the experimenter's observation. These practical difficulties appear to force one to the conclusion that the psycho-galvanic reflex, useful as it may be for diagnostic purposes, is not at present a suitable method for exact investigation.

Secondly, even supposing that it were safe to assume that the deflexion shown by the galvanometer accurately recorded the emotional reaction to the given stimulus, we could not conclude, from the absence of correlation, that the aesthetic judgment was not based on emotion. The judgment might be determined not by the *amount* of the emotion aroused by the object, but by the quality of the emotion: it might, for instance, be based on one particular emotion,—the so-called 'aesthetic emotion' may actually exist.

Yet another alternative appears to be open. It may be that the aesthetic judgment sometimes has, and sometimes has not an emotional basis. For example, Subject X, one of the most reliable of the subjects, that is, one about whom the experimenter could be reasonably sure that the deflexion showed the reaction to the given stimulus, gave big emotional reactions to certain cards which suggested Eastern objects, and these same cards evoked very favourable judgments; *i.e.* the emotional significance of the card for the subject seems to have determined a favourable aesthetic judgment. On the other hand, she gave no reaction at all to two cards which she liked very much indeed but which did not suggest Eastern objects and so had not the emotional significance of the others. The results obtained in the first series of experiments would not be opposed to the view that in the case of some individuals the aesthetic judgment is based on emotion while in others it is not; and the results obtained from Subject X in Series II at least suggest that the aesthetic judgment may sometimes be determined by the emotional significance of the object and sometimes not. It is at least a possible alternative left open by the results of these experiments that there may be both an intellectual and an emotional aesthetic judgment. Such a view would be opposed to the view that there is a peculiar 'aesthetic emotion': it would hold that there is no one condition which an object must fulfil if it is to be judged 'beautiful'; a position of some interest for philosophical aesthetics. Certainly all experimental aesthetics bears witness to the markedly different ways in which an object may appeal to different individuals and yet be liked or judged beautiful by each: for example the difference between the objective and subjective or character types, and between the pattern- and picture-types; for it is difficult to assert confidently that any one type is the only one to reach the true aesthetic attitude. If it could be shown that sometimes the aesthetic judgment has, while sometimes it has not an emotional basis, it would be interesting to know which type, objective or subjective or character, pattern or picture, gives emotional aesthetic judgments and which intellectual. But on this question, of course, the present experiments throw no light at all.

IV. SUMMARY OF RESULTS.

1. The order of preference for coloured rectangles differs markedly from that for uncoloured rectangles, a fact which points to a greater interest in colour than in form where the material is simple. There is a similar difference where rectangular surfaces are substituted for outlines but not nearly so marked.

2. The Golden Section rectangle holds a high place where the rectangles are uncoloured, but its popularity is not sufficiently great to outweigh the dislike of its colour when the rectangles are coloured.

3. The four 'perceptive types' which Mr Bullough demonstrated in the aesthetic appreciation of simple colours and colour-combinations and Dr Myers in the aesthetic appreciation of tones, appear also in the aesthetic appreciation of simple forms.

4. The chief result of the experiments with arrangements of geometrical forms is the demonstration of two distinct attitudes, that regarding the designs merely as formal arrangements of figures, and that regarding them as representations of objects or scenes. Subjects adopting these attitudes have been said to belong to the 'pattern-type' and the 'picture-type,' respectively. These two types appear both in the construction of the designs and in the observation of the designs made by others. The pattern-type appears to be more objective and formal, the picture-type more subjective and imaginative.

5. In the observation of the designs, whether a particular design

appears as a pattern or a picture is determined both by objective and subjective factors.

6. Mr Bullough's four 'perceptive types' occur also with the more complex material, but all judgments cannot be brought under one or other of the four types. Three other classes are found, viz. judgments referring to the artist, those referring to the 'imaginativeness' of the design, and those referring to the presence or absence of meaning.

7. Roughly it may be said that the majority of pattern-type judgments are of Mr Bullough's 'objective' type, while the majority of picture-type judgments are of his 'subjective' or 'character' type.

8. With the increasing complexity of the material, form assumes an increasing importance relatively to colour. This is more pronounced in subjects of the picture-type than in subjects of the pattern-type. The two types differ somewhat in their attitude to colour, the one aiming chiefly at a realistic arrangement of the colours, the other at harmony of colour.

9. The fact of having constructed a particular design does not necessarily determine a favourable aesthetic judgment on that design.

10. No evidence was obtained to support the view that the aesthetic judgment is based on emotion: there was no general correspondence between the aesthetic judgment and the emotional reaction as measured by the psycho-galvanic reflex. In view of the practical difficulties of the method, particularly that of knowing how much of the reaction is due to the given stimulus and how much to simultaneous stimuli, the results must be considered ambiguous. Moreover it is not impossible that the aesthetic judgment may depend not only on the amount but also on the quality of the emotion, and that there may be intellectual as well as emotional, aesthetic judgments. The practical difficulties of the interpretation of the deflexions suggest that the psycho-galvanic reflex, useful as it may be for diagnosis, is in its present form, at least, not suitable for exact psychological investigation.

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LEFT-HANDEDNESS AND THE REVERSAL OF LETTERS.

By L. G. FILDES and C. S. MYERS.

I. *Experiments* (pp. 273–276).
II. *General results* (pp. 276, 277).
III. *Conclusions* (pp. 277, 278).

I.

THE brief series of experiments here described was conducted by one of us (C.S.M.) in 1913 on a markedly left-handed boy, whom we will call E., between six and seven years of age. His maternal grandfather, his maternal uncle, and a son of that uncle are also left-handed. The object of these experiments was to throw light on E.'s peculiar use of, and apparent preference for, the 'reversed' (*i.e.* mirrored) forms of letters and numbers. At the time E. knew some, but not all, of the letters of the alphabet, and all the ten unit arabic numbers. He had just begun to be taught to write with his right hand; but he found the greatest difficulty in doing so, owing to the coarseness and the want of co-ordination of the movements of that hand. For these reasons, and especially because he began to stutter, he was soon allowed to write with his left hand only; whereupon the confusion of the positions of letters and the disturbance of speech rapidly disappeared.

The experiments are obviously incomplete, but in view of the present interest in the subject[1], their publication seems justified. They fall into three main groups, each group dealing with a special aspect of the general inquiry. The aim of each of these groups, and the results obtained therefrom, will be described in turn. Finally, the general results and conclusions suggested by the whole of the observations will be summarised.

Group I. The experiments in this group were planned to discover how far the presentation of numbers and letters (*a*) in the 'ordinary' position, (*b*) 'reversed' (mirror fashion), and (*c*) 'inverted' (upside-down or recumbent), affected E.'s ability to read them. With this object, on

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[1] Cf. H. Gordon, "Left-handedness and Mirror Writing..." *Brain*, 1920, XLIII, 313–368.

March 25, the following series of numbers (E. took a natural interest in arithmetic) was shown him:

6 1 *2* 4 *6* 7 9 *3* *5* 2 *4* *7* 8 *9* 3 5

of which those here printed in italics were drawn 'reversed.'

All were named by him with equal ease; no delay or hesitation being perceptible in naming the reversed numbers in the series. The numbers ordinarily written and in their reversed positions were described by E. as "the same, turned different ways." He could, however, be induced to *choose* the rightly placed numbers, except in the case of 5; here he preferred the reversed form and considered it 'right.'

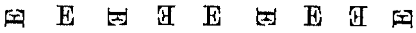
Later (June 17) four series of *letters*, differing in length, were shown, in the ordinary, reversed and inverted positions, and the reading times were taken. These times are given in seconds, in the following table:

Number of letters in series	4	4	10	24
Ordinary position of letters	3	4	10	34
Reversed „ „	3	3	11	34
Inverted „ „	6	5	—	—

Thus, the recognition of letters, like that of numbers, was effected with approximately equal ease whether the material was presented ordinarily or in reversed form. Indeed, E. once remarked that "it is easier to read in the glass"! Inversion of the letters made recognition more difficult; this difficulty was felt by E. who observed: "The upside-down ones are the hardest."

Group II. The aim of this group of experiments was to ascertain how often the 'right' position would be chosen visually in the case of certain letters of the alphabet, already fairly known—b, C, D, E, h, J, K, L, S—when each of them was shown to him in different positions. For this purpose E. was given practice from March 29 to April 3, in learning to draw them with his left hand and to name them, until he could easily recognise them in their ordinary positions. At the same time two 'artificial' letters, one of which was called 'goo,' and the other 'ree,' were similarly taught him.

On April 4, May 27 and June 4, E. was required to "choose the most correct letter" in different lines, each showing a letter in varying positions, as, for instance, in the line following:



On April 4 he repeatedly and consistently chose the reversed forms of b, h, and 'ree,' as being the 'right' positions. Later, however, on May 27 and June 4, after first selecting the reversed forms of E, C, D, S, J, K, L, he chose both the ordinary and reversed forms of J, S, C and E. These forms, ordinary and reversed, were termed by him "the same thing." Indeed, he seemed quite unable to detect any difference between the two positions (normal and mirrored) of E or D, until it was suggested to him that "one looks one way, the other the other." Even then he could not decide which way these letters *ought* to look, so that he finished by saying: "I should choose both." Usually the inverted (the upside-down and the recumbent) letters were rejected; he called the recumbent forms of K and E, and , "upside-down."

On April 6 he was asked to write the letters h, 'goo,' b and 'ree' with his left hand (*i.e.* as they were learnt); he wrote h and 'goo' correctly, l for b, and reversed 'ree.' He considered l the "same" as b[1]: "it only needs a —." His reason for reversing 'ree' is interesting: the drawing was begun correctly, but was altered to the mirrored position, because "it *looks* better; all the other letters face this way."

On June 4, the letter Z, previously unknown to E., was correctly learnt by drawing it blindfold with guided left and right hands, alternately. On the following day, he showed the same confusions as before; specimens of Z, whether ordinary, reversed, or recumbent, were "all zeds," when he was asked to make a visual choice; though ultimately, on his being pressed to be careful, he rejected the recumbent (which had been his first selection) as being an 'en.' Moreover, after E. had correctly learned the unseen form of an 'artificial' letter (known to him as 'shoo') on June 4, 6 and 14, by repeatedly drawing it blindfold with the guided left hand, the *reversed* form was *visually* chosen several times on June 20 as the most correct, whereas on June 15, without practice since the previous day, he had drawn the *correct* form *blindfold* both by the left and by the right hands. So, too, the drawing of Z was *reversed* on the introduction of the *visual* factor: E.'s *blindfold* drawing of it on June 5 was Z both with the right and with the left hands, but he drew it *reversed* with either hand *when his eyes were open*[2].

Group III. An attempt was now made to ascertain how far E.'s tendency to visual choice of the reversed position of a letter was affected by the method of learning. With this object ten letters were taught him, (*a*) two letters, visually, (*b*) three letters, visually and with right-hand drawing, (*c*) three letters, visually and with left-hand drawing, (*d*) one letter, blindfold and with right-hand drawing, (*e*) one letter, blindfold and with left-hand drawing, until they could be reproduced correctly—blindfold, in the case of (*d*) and (*e*)—and their names were known.

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[1] In the case of b and h, he was taught the hand-written, not the printed, form.

[2] In learning 'shoo' (which consisted of four short lines, each at right angles to the preceding one) by blindfold drawing, evidently E. first mastered the number rather than the direction of the successive movements. Thus, on June 6, before the day's practice, he reproduced it as four lines successively at right angles, but making a letter quite different in form from that taught him.

This group of experiments was carried out from September 17 until October 5, various letters being practised every day; and on September 23, 25, 27, 28, 29, October 3 and 4, certain tests were interpolated. These tests were similar to those used in the experiments of Group II. At each test E. was asked to pick out the 'right' letters, pointing to his choice.

Unfortunately the results cannot be formulated quantitatively inasmuch as (i) most of the letters used had been at least partially learnt before the experiments began, and no precise information is available as to the methods or the effects of such learning; (ii) the number of repetitions needed for learning differed according to the letter; (iii) too few experiments could be made with methods (*d*) and (*e*) to have any value. But so far as they go, the data indicate the distinct (from 25 to 50 per cent.) superiority of method (*a*) over any of the other four methods, and the superiority of method (*b*) over method (*c*). That is to say, visual choice was more correct after purely visual learning (though even here there were 30 per cent. of wrong choices), and was more adversely affected, in the direction of preferred reversal, by left-hand than by right-hand drawing practised with the eyes open. Blindfold drawing led rather to confusion of upright with recumbent positions.

Two points, observed during these tests, are noteworthy. (i) In choosing the letter C (practised blindfold with left-hand drawing), E. was uncertain of the 'right' position, until he was allowed to run with a pencil over the variously placed letters in the test. With the aid of such left-hand tracing, the 'right' forms of the letter were chosen; whereas on subsequent right-hand tracing the reversed forms were chosen. (ii) Inversion of the test-sheet led to changes in E.'s choice—generally to choice of the corresponding reversed position. This E. explained as due to the fact that he thought that when the paper was inverted the letters must "look the other way."

II.

The foregoing experiments show that:

1. E. could read ordinary and reversed letters and numbers with about equal ease, but read inverted letters more slowly.

2. He had the greatest difficulty in deciding visually "which way a letter should go." He would frequently—invariably in the earliest ex-

periments—*choose* the reversed positions of letters shown him which he had previously learnt with open eyes by (especially left-hand) drawing them, although he could *reproduce* them correctly.

3. The purely visual method of learning letters was found to give the greatest number of subsequent correct visual choices, but even then only about 70 per cent. of the choices were correct.

4. The *reversed* position of a previously unseen letter or form, which E. had learnt correctly to draw *blindfold* with either hand, was preferred not only (i) on the visual presentation to him of a choice of positions, but also (ii) in subsequent drawing with either hand when the eyes were *open*.

5. After such blindfold learning of an unseen letter, repetitions of the movements by the learning hand traced over the letter visually presented in different positions enabled a correct choice to be made; whereas the reversed form was preferred when the tracing movement was performed by the opposite hand.

6. Right-hand drawing practised with the eyes open seemed to favour more correct visual choice than left-hand drawing.

7. A letter correctly learnt by one hand blindfold was not (at all events, necessarily) reversed when subsequently drawn blindfold by the other hand.

III.

These results seem to warrant the following interpretations:

1. E.'s choice and writing of a reversed letter instead of an ordinary letter are the outcome both of visual and of manual influences.

2. A child's early visual experience is probably little concerned with the *absolute* position of seen objects. His attention is first drawn to their form, and his powers of recognition are not gravely disturbed, whether that form once learnt, be re-presented to him in the 'ordinary' or 'reversed' positions or even in the 'recumbent' or 'upside-down' positions; he apprehends the letter, as it were, in a definite visual 'schema'—through the inter-relation of its parts within the whole.

3. Whereas the visual learning of a letter rapidly permits of a simultaneous image of its parts, manual (blindfold) practice at its reproduction affords for some time at first a memory of the successive movements required to draw the letter. But finally, a *motor* 'schema' may be obtained which enables the child to reproduce the letter correctly (blindfold), even with the opposite hand.

4. But before this stage is reached, and while the visual and motor schemata are being integrated, considerable confusion must arise from

the tendency for one hand to perform the reversed (mirrored) movements learnt by the other. Especially must this confusion occur in the case of a left-hand child, like E., whose right and left hands are being both employed in writing. Since, for example, the letter E, when learnt with the right hand, is at this stage most readily written Ǝ with the left, it is evident that in a left-handed child, taught to write with his right hand, and writing with his left when unobserved, a special factor is operative which tends to present him with the wrong (*i.e.* the reversed) form more frequently than in the case of a right-handed child, and so must lead to utter impotence to decide which is the 'right' form of the letter. In the case of such a left-handed child, this special factor may cause him actually to prefer the reverse to the direct forms when presented to him, especially after he has learnt to draw them correctly with his left hand; it may even lead him, when drawing with his eyes open, to reverse a letter which he has already learnt blindfold, with either hand to draw correctly.

5. The better results of right-handed than left-handed drawing with the eyes open, in the case of a left-handed child, are too difficult to interpret here.

(*Manuscript received* 10 *November* 1921.)

FACTORS IN MENTAL TESTS.

An abstract of an experimental investigation.

By E. J. G. BRADFORD.

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If it be granted that the various forms of intelligent behaviour may be related to one another in virtue of the units of behaviour of which they are composed, it seems reasonable to suppose that mental and physical tests should lend themselves to being similarly related, in virtue of the units of behaviour which are involved in successful reaction thereto. The purpose of this investigation was to demonstrate the possibility of a quantitative relation being established between groups of such tests. The task of examining tests with a view to relating them into a system is rendered difficult, however, by many causes, some of the more obvious being (i) the labelling of tests with psychological labels derived from introspective study alone; (ii) the relationship assumed to exist between 'ability' and 'capacity'; (iii) the differential improvability (educability) shown by the subjects tested; (iv) the individual variability of the subjects due to causes which are not under the control of the subject or of the experimenter; (v) the interpretation of the reliability coefficients obtained; (vi) the need of a theory concerning the mode of combination of the units or factors.

Evidence bearing both on the main problem and on the difficulties cited above is furnished by experiments which were carried out with six approximately equal groups of children (three groups of boys and three of girls) totalling in all 224 subjects. Six tests were given on each of ten occasions during one month. The tests used were—Opposites (O.), Backward Alphabet (B.A.), Forward Alphabet (F.A.), Schema (S.), Ring (R.), Ring Motor (R.M.). The last three tests have been described respectively as the Spot Pattern, B. and A. tests in a paper by Dr Reaney[1]. Two control tests were given twice each toward the end of the month—the Dot Chain (D.C.) which consisted of crossing out dots arranged irregularly in a column, and the Handwriting Speed (H.W.) which consisted of writing out the alphabet as rapidly as possible.

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[1] This *Journal*, 1914, VII, 235.

1. Tests demanding a marked degree of concentrated attention give no correlation amongst themselves, *e.g.* R. and B.A. give an average of ·12 ± ·08. Of 450 correlations calculated between the different applications of the six tests only 14 negative ones are as large as their P.E., suggesting that factors correlate positively or not at all. If this result be true generally then the low correlation mentioned above cannot be ascribed to the influence of factors acting in opposition to an assumed 'attention factor'; it is low probably because attention is specific to the activity involved in the test and varies with it.

2. The average correlation between initial and final ability is only ·66; the correlation between initial ability and capacity is considerably lower. An average improvement equivalent to 100 per cent. in the mean performance was shown after ten attempts. The amount of improvement varies considerably among individuals and is the chief influence tending to lower the correlation between initial and final ability.

3. The average correlation between improvability (educability) and initial ability is zero, but with final ability it is ·59. The elimination of the influence of differential improvability is not achieved by an amalgamation of all the applications of a test; continued applications will tend to increase the reliability up to the point where the individual limits of capacity are reached, at which point individual variability alone will influence the reliability.

4. The existence of improvability necessitates the measurement of individual variability about a progressive mean; this measure[1] of variability can be regarded as an inverse measure of the consistency of performance. The average correlation between individual variability and improvability is − ·22. In other words it may be said that consistency and educability correlate to the extent of ·22. Owing to the influence of individual variability, the reliability coefficient between proximate applications of the tests seldom exceeded ·85.

5. Both improvability and variability tend to lower the reliability coefficients and to mask the true relation existing between the tests. This relation appears to be constant, irrespective of the gradual increase in the reliability coefficients, as an inspection of the following table will show.—

Tests correlated	Initial application	Final application	Tests correlated	Initial application	Final application
B.A.—F.A.	·66	·66	R.M.—R.	·58	·57
F.A.—O.	·40	·49	R.—S.	·32	·32
B.A.—O.	·33	·34	R.M.—S.	·25	·26

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[1] Cf. "Measures of Variability," *Amer. J. of Psychol.*, XXVII, 234 ff.

The above uncorrected average values represent a quantitative relation which is not fully assessed but needs correction for the variability of the subjects tested. The reliability coefficient was treated as the personal equation of the group tested, and a correction formula elaborated which gave results almost identical with the Spearman correction formulae.

6. On the basis of correlation values and introspective evidence, certain tests were judged to be relatively simple and others complex. It was assumed that the correlation between the simple and complex tests could be likened to the correlation between two series of dice throws, one of which series was wholly included in the other, the size of the coefficient depending on the relative magnitude of the uncommon factor[1]. The factors were expressed as percentage values, thus:

D.C. $= 100\,M$
S. $= 100\,P$
F.A. $= 100\,R$
O. $= 80\,R + 20\,M$
R. $= 80\,M + 20\,P$
R.M. $= 48\,M + 52\,P$
B.A. $= 42\,R + 58\,D$
H.W. $= 54\,R + 46\,M$

The factors Motor, Apprehension (Schematic), Recall and Dissociation (a doubtful factor) are represented respectively by the letters *M*, *P*, *R* and *D*.

The validity of the assumption that the 'mental' factors (if they may be so termed) may be treated after the manner of dice, is confirmed by the correlations between the complex tests. The theoretical coefficients calculated from the above formulae correspond closely with the actual correlations obtained after correction for variability. In the table below, column *A* gives the actual correlation corrected for variability, and column *B* gives the correlation calculated from the derived formulae.

Tests	*A*	*B*	Tests	*A*	*B*
R.M.—R.	·71	·70	B.A.—O.	·49	·58
H.W.—O.	·68	·69	B.A.—H.W.	·43	·44
R.M.—H.W.	·58	·66	R.M.—O.	·41	·40
R. —H.W.	·57	·51	R. —O.	·30	·31

A general law, regarding the mode of combination of factors, is suggested by these results, viz. *that the extent to which simple tests correlate with similar tests of more complex nature may be used as a basis for determining the degree to which the same complex tests will correlate among themselves.*

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[1] Cf. *J. of Exp. Ped.* v. 297.

(*Manuscript received* 5 *November* 1921.)

A NOTE ON THE USE OF THE PSYCHOGALVANIC REFLEX.

By W. WHATELY SMITH.

(*From the Cambridge Psychological Laboratory.*)

There can be little doubt that the psychogalvanic reflex is the most delicate method yet devised for the detection and measurement of affective tone, and inasmuch as the latter appears to be the most important determinant of all mental states, it is clearly desirable to refine, by every means in our power, any technique adapted to its quantitative study.

One of the chief difficulties connected with the use of this method is that of making comparable with one another the reactions observed in different subjects and on different occasions. It might be supposed that the absolute magnitude of the reflex produced by such physical stimuli as pricks, burns, sudden noises and so forth, would afford an indication of the comparative 'emotivity[1]' of the subject concerned, and that this might be used as a 'vocational test' for occupations demanding self-control. But it was soon realised that factors other than emotivity greatly affect the absolute magnitude of the reflex which is, therefore, of small value as a test of that quality.

It is with this question of the comparability of reactions that the following observations are mainly concerned.

The phenomenon appears to be a very complex one and we are at present far from a thorough understanding of its mechanism. There seem to be, for instance, at least two clearly distinguishable forms of the reflex; first, a change in the effective resistance offered by the skin to the passage of an electric current and, second, a generated electromotive force which is independent of any current applied *ab extra*.

Of these two varieties the former is certainly a skin effect, though whether it is due to a change within the skin itself or to a change of polarisation at its surface is not yet clear. It is with this form that I shall concern myself below.

Many methods have been used for studying the reflex. Following Waller, I myself have always used a Wheatstone's bridge and D'Arsonval galvanometer in conjunction with two zinc-plate electrodes, covered with wash-leather and soaked in a concentrated solution of common salt; these were applied to the palm and back of the subject's left hand which thus formed the external resistance of the bridge.

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[1] 'Emotivity' being here used to denote liability to react to an exciting stimulus.

The following factors appear to be involved in determining the absolute magnitude of the galvanometer deflexion produced by a given stimulus:

(i) The intensity of the emotion actually evoked.

(ii) The proportion of it which finds expression through those efferent channels which innervate the skin-mechanisms responsible for the reflex.

(iii) The responsiveness of the skin to such innervation[1].

(iv) The initial resistance of the skin.

(v) The sensitivity of the galvanometer.

(vi) The magnitude of the fixed resistances of the bridge.

(vii) The E.M.F. applied to the bridge.

Of these, (i) is the quantity which we wish to measure, (v), (vi) and (vii) are easily kept constant or, if not, suitable corrections can be made on their account. Of the remainder, (iv) can readily be measured, and I deal below with the appropriate correction for it, but (ii) and (iii) are variables for which, at present, no allowance can be made.

I propose to deal here with the question of what correction should be applied to compensate for variations in the initial resistance of the skin.

This point is of importance for two reasons. First: even if we cannot eliminate all the causes of variation, other than (i) above, between different subjects, it is desirable to remove as many as we can, both with a view to closer study of those which remain and in order to reduce the amount of fortuitous variations to be neutralised by the use of such statistical methods as may be necessary. Second: if we are studying the behaviour of the same subject on different occasions we shall wish to make the results obtained as comparable as possible with respect to (i) and we may for the present assume that (ii), at least, and perhaps (iii) are not likely to vary greatly in the same subject from time to time.

One of the obvious results of a difference in resistance between two subjects will be that a heavier current will be passed through the subject of lower resistance than through the one of higher resistance (assuming the E.M.F. on the bridge to be kept constant).

It is easy to show experimentally that, in general, the greater the current passed through the subject, the greater is the absolute magnitude of his reactions. The question then arises whether this increased reaction is due simply to what I may call 'normal' electrical causes or whether, as has been suggested by Prideaux, the heavier current produces some definite effect on the subject of such a nature as to increase his 'irritability' quite apart from the increased deflexion which would be expected on purely electrical grounds. In other words, can the living subject be treated, so far as differences of initial skin resistance are concerned, as if he were an inanimate resistance whose changes we were observing?

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[1] I think it probable that this factor may, for all practical purposes, be subsumed under (ii) or (iv) or both.

This would be easy to determine if we could apply standard stimuli to subjects of different resistances and measure the deflexions produced. This procedure, however, appears to me impracticable, partly because a stimulus of small emotional import to one subject may arouse intense emotion in another and partly on account of possible and unknown effects due to the factors (ii) and (iii).

These difficulties can, however, be largely surmounted by the use of appropriate statistical methods.

In connexion with some experiments on memory and affective tone described in an earlier paper[1], I had occasion to apply a word-association test of 100 words to 50 different subjects, and to compute the mean resistance of each for the period of the test. I therefore first calculated the coefficient of correlation between the mean galvanometer deflexion and the mean resistance for these 50 subjects. Its value was $-\cdot497$; that is to say, there is a strong tendency, as we would expect, for deflexions to increase as resistance decreases.

On general grounds it seemed probable that initial resistance and the deflexion produced by a given stimulus would be connected by a relation of the form

$$R^x D = K$$

when R is the initial resistance, D the deflexion and K a constant. I therefore calculated the values of the coefficient of variation for the expression, $R^x D$, for this series of 50 subjects, giving x the values 0, 1 and 2 successively.

The resulting values are:

x	C of V,
0	·656,
1	·572,
2	·704.

These values lie on the curve $V = \cdot098x^2 - \cdot182x + \cdot656$, which has a minimum at the point $x = \cdot925$; $V = \cdot5717$.

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[1] This *Journal* (Gen. Sect.), 1921, XI, 236.

That is to say the effect on the absolute magnitude of deflexions, of differing resistances of the subjects, can be more perfectly removed by multiplying the deflexions by $R^{\cdot 925}$*—which is substantially equal to R—than by any other power of R.*

(It may be noted that the improvement effected by using the expression $R^{\cdot 925}D$ instead of RD is inappreciable, for the coefficient of variation only changes from ·572 to ·5717.)

As a check on this, I substituted a resistance box for the subject and obtained by direct calibration the deflexions corresponding to a constant percentage decrease in resistances of 1, 2, 3, 4, 5, 6, 7, 8, 9 and 10 thousand ohms. Similar treatment of these gave an optimum value for x of approximately ·9. We may therefore conclude that so far as variations of initial skin resistance are concerned, the subject does behave in substantially the same way as an inanimate resistance, and that the differences observed as the effect of passing a larger or smaller current through the subject are wholly due to normal electrical causes and not to any further effect of the current upon the subject himself.

It is not always necessary to apply this correction; but in cases where it is desirable to do so, the inconvenience of actually multiplying each deflexion by the resistance of the subject can be obviated by any one of the three methods described below.

(i) If we take a subject of resistance 1000 ohms, say, as 'standard' and use for such a subject a galvanometer shunt of x ohms selected so as to give deflexions of suitable size for ordinary stimuli (producing a decrease of resistance of about 2·5–3·0 per cent.), it is easy to obtain by calculation or, preferably, by direct calibration, the values of the shunts which will give the same deflexion for the same percentage decrease of resistance in the case of subjects of resistances 2000, 3000, 4000, etc., ohms. These values can be plotted graphically as ordinates against resistances as abscissae and the shunt appropriate to a subject of any resistance can be read off from the resulting graph.

(ii) Another method is to use no shunt on the galvanometer but to control the magnitude of the deflexions by varying the E.M.F. applied to the bridge by means of a potentiometer. Here again, the best procedure will be to calibrate the apparatus directly by substituting a resistance box for the subject, balancing resistances of 1, 2, 3, 4, 5, etc. thousand ohms on the bridge, reducing each of these when balanced by the same percentage—2·5 say—and adjusting the potentiometer so as to give the same deflexion in each case. Potentiometer adjustments can then be plotted against resistances as before.

(iii) A third method, which has the advantage that it requires considerably less apparatus than either of the foregoing, is to abolish the use of the bridge altogether and to employ a modification of Binswanger's arrangement described in Jung's *Studies in Word Association*, p. 446. The subject, battery and electrodes are here placed in series and no bridge is used[1].

When the subject is at rest and not stimulated, there will, of course, be an initial steady deflexion of the galvanometer. Assuming that the resistance of the latter and of the remainder of the circuit is small compared with that of the subject, and that the galvanometer deflexions are proportional to the current over the range in question, the deflexion will increase by a percentage equal to that by which the subject's resistance decreases.

Thus, for a subject of resistance 5000 ohms, the initial deflexion will be twice as great as for one of 10,000 ohms, and so will the added deflexion corresponding to any given stimulus.

If we interpose a potentiometer between the battery and the circuit, we can always pass the same current through the subject, thereby producing a constant initial deflexion and constant subsequent deflexions for the same percentage decrements in the subject's resistance, whatever the absolute magnitude of the latter may be. This is what is required.

The only disadvantage of this method is that the galvanometer always starts with a large deflexion of which only a small percentage increase is observed as the result of stimuli. In its simpler form it has, however, been successfully used by Binswanger, Veraguth and others, and its simplicity and cheapness are very much in its favour.

As I have observed above, it is not always necessary to apply a correction for the resistance of the subject by any of these means or by direct multiplication. The way in which results are handled and the form in which they are expressed should depend upon the objects of the experiments. Thus, if we are using the reflex merely as a 'complex indicator' in a word-association test—as a preliminary to psycho-analytic treatment, for example—no corrections of any sort need be applied, for all that concerns us is the *relative* degree of emotion evoked by the various stimulus words. If, on the other hand, we are seeking to ascertain which words of a list, or which members of a series of other stimuli provoke most emotion, on the average, in a given class of subjects, it will be best to express the reaction to each stimulus as a percentage of the mean reaction of the subject concerned for all the stimuli. Thus, if ten stimuli are applied to a given subject with the following results:

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[1] An alternative arrangement combining the advantages of this with those of the bridge mentioned has been described by Prideaux (*Brain*, 1920, XLIII. 50–73).

Stimulus	A	B	C	D	E	F	G	H	I	K
Reaction	7	3	9	14	2	11	8	6	12	5

of which reactions the arithmetic mean is 7·7, we should express the results for every such subject as follows:

Stimulus	A	B	C	D	E	F	G	H	I	K
Reaction (% of mean)	91	39	117	182	26	143	104	78	156	65

This eliminates not only variations due to resistance but also the danger of the results being unduly influenced by excessively large or small reactions, of whatever origin, on the part of a single subject.

Whether the arithmetic or the probable mean should be used will depend on circumstances; in nearly all cases the latter is preferable. But if we wish to compare the behaviour of different classes of subjects with respect to the psychogalvanic-reflex in general, it will be necessary to apply the correction for resistance. For the classes may differ by virtue of the factors (ii) and (iii) mentioned on p. 283 above, and this may be important. For instance, if we are comparing normal with mentally deficient persons, it may be, as is suggested by some as yet unpublished experiments by Prideaux, that the latter persons give very small reactions to all classes of stimuli because only a small proportion of the emotion aroused finds expression through the mechanisms responsible for the reflex, or because they have skins of resistance much higher than the normal, or because they really feel less—*i.e.* less emotion is actually aroused.

If the uncorrected results were simply averaged for a number of such defective persons and compared with the similarly treated results for normal persons, we could form no definite conclusions on the subject. Whereas, if due allowance is made for variations in skin resistance, any difference between the size of the reactions given by the two classes of subject can either be ascribed to this cause or, when it is eliminated, shown to be due to one or more of the others.

Finally, inasmuch as the phenomenon consists essentially in a lowering of the resistance of the skin, and as the percentage decrease of resistance appears to be, very approximately, directly proportional to the intensity of the emotion aroused, it is desirable that all results which are published with a view to comparison with those obtained by other experimenters should be expressed in terms of percentage decrement of resistance or, at least, that sufficient data should be given to enable the results to be reduced to these terms. Absolute deflexions are valueless for comparative purposes, as they depend so largely on the particular arrangement of apparatus used.

It may be of interest to note that in the case of the 50 subjects mentioned above to whom I applied a word-association test, the mean resistance for all subjects was 4400 ohms and the mean deflexion 7·32 mm. This corresponds, with my apparatus, to a decrement of resistance of about 2·3 per cent.

(*Manuscript received* 10 *October* 1921.)

A COMPARISON OF SPEED WITH ACCURACY IN THE LEARNING PROCESS.

(*A Report to the Industrial Fatigue Research Board.*)

BY MARY STURT.

(*From the Cambridge Psychological Laboratory.*)

I. *Aim of the investigation: conflicting views on the relative importance of speed and accuracy in habit-learning* (pp. 289, 290).
II. *Description of the experiment: two groups of subjects, the one working for speed, the other for accuracy; tests for insuring equal mental ability of the two groups* (pp. 290–294).
III. *Results: profound differences in the two groups when typing sensible matter; effects of reversing the instructions* (pp. 294–297).
IV. *Discussion: error of Gilbreth's views; pedagogical fallacies; initial and final insistence on accuracy* (pp. 297–300).
V. *Conclusions.*

I. AIM OF THE INVESTIGATION.

THIS experiment was undertaken with the object of obtaining evidence concerning the question, whether, in learning habits involving muscular activity, it is better to insist from the start on accuracy or on speed. This point has only recently become a subject of controversy. Formerly it was assumed that the 'accuracy method' was the only feasible one, and that an almost exaggerated attention to details at the start was the only way to achieve skill finally. Such a view is well expressed in the following advice given by Southward[1] to those learning hand-compositing:

> The rule for apprentices should be—first study accuracy and an easy position and regard speed as a secondary consideration. There is more danger in going too fast than too slow.

It is clear from the whole of the above passage that Southward would insist on the beginner working slowly at first, because he believes that only by close attention to a movement can the beginner succeed in making it correctly, and without correct movements it is impossible to obtain speed subsequently. The ordinary school emphasis on accuracy and neatness seems to be due partly to an underlying belief in the *moral* value of tidy work and in the necessity of 'cultivating an accurate habit of mind,' but partly also to the conviction that inaccuracy, once tolerated, can never be removed.

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[1] Southward, *Modern Printing*, 1915, p. 160.

Quite recently, in his *Applied Motion Study*, Gilbreth has delivered a frontal attack of much violence on this long-accepted position. He points out that slow movements are inevitably different *in form* from (the 'same') fast movements, and contends, therefore, that if the beginner starts by learning movements slowly in order to make them *accurate*, he is simply *not* learning the *fast* movements that are finally required.

To this question of speed he attaches great importance, returning to it several times:

> Right motions must be insisted on from the beginner's first day of work. Standard speed of motions must be insisted on from the learner's beginning on his first day, if least waste in learning is to be the first consideration[1].

And again:

> The learning process is the proper repetition of the desired method at the standard speed. The proper insistence is upon the right methods at standard speed first and the quality of work produced second[2].

He explains that by standard speed he means the speed at which the maximum of work can be produced. If his methods are followed, he feels sure that accuracy will ultimately be attained. If this position is to be accepted, it clearly involves a method of teaching totally different from that in general use at present, and it is of considerable importance to industry.

II. DESCRIPTION OF THE EXPERIMENT.

The general plan of the experiment was that two groups of subjects, as nearly equal in ability as possible, should learn some activity, one group by the 'accuracy method' and the other by the 'speed method,' and that then the results should be compared. Typewriting was chosen as the work to be learnt as it seemed easier to get subjects for this than for a 'useless' activity, such as some motor test. The subjects were two groups of ten each, all girls, between the ages of 12 and 14, attending two Cambridge schools, R. and P.[3]. The R. group learnt by the speed method, the P. group by the accuracy method. Owing to the fact that different instructions were given to the two groups (see below), it was essential that members of one group should not discuss the experiment with members of the other, and for this reason the groups were obtained from different schools. Each group of ten was selected from a much larger group at its school, with the object of obtaining two *equal* groups. The facts considered in making this selection for equality were (1) age, and (2) performance in several mental tests. (The social status of one group seemed identical with that of the other.) These data are set out in Tables I and II, the tests used being as follows.

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[1] *Op. cit.* p. 109.

[2] *Op. cit.* p. 121.

[3] I must thank the Cambridge Borough Education Committee for facilities for the experiment, and the headmaster and mistresses of the schools for their great kindness throughout.

(1) *A Substitution Test* (digits for letters). This test was given twice; on the first occasion (*A* in Tables I and II) a longer time was allowed for it than on the second (*B* in Tables I and II). The score used was the actual number of substitutions made in the time allowed. The figures for this test in Tables I and II are these scores.

(2) *The Woodworth and Wells Opposites Test.* The score was the number of opposites written in one-and-a-half minutes, 40 words being used in the test. The figures for this test in Tables I and II are these scores.

(3) *A Trabue Completion Test.* Each blank filled in correctly was scored = 1. The figures for this test in Tables I and II are the total scores.

(4) *An Immediate Memory Span Test.* The material consisted of a number of sentences, written sufficiently large to be exposed to the whole group at once. Each sentence was shown for 10 seconds. The score was the total number of words correctly reproduced. The figures for this test in Tables I and II are these scores.

(5) *A Spelling Test.* The material consisted of a selection of 50 words from Burt's test[1]. The score was the number of words correctly spelled. The figures for this test in Tables I and II are these scores.

Of these tests, the *substitution* test was used because it has been considered a good test for typewriting capacity[2], though it is doubtful if this is so. The *opposites* and *completion* tests were used because they are among the best 'general ability' tests. The *immediate memory span* test was used because the function it brings into operation seemed important for typewriting. The reason for the *spelling* test was that errors in typewriting are often due simply to faulty spelling, so that if one group were here clearly superior to the other it might have a certain advantage over it. It will be seen by comparing the figures in Tables I and II that this was the one test in which the groups did not appear to be entirely equal, the

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[1] See Ballard's *Mental Tests*, pp. 157, 158.

[2] See *Industrial Fatigue Research Board Report*, "On Vocational Guidance," section on Typewriting (now in press).

P. group being apparently slightly superior to the R. group in spelling capacity. Partly to prevent this difference from interfering with the results, the copy material chosen for teaching was such as involved a minimum of spelling difficulty.

TABLE I. R. GROUP.

	The tests						
Subjects	Substitution		Opposites	Completion	Memory	Spelling	Age
	A	*B*					
S. E.	379	210	27	23	94	42	13·2
I. C.	360	225	29	29	87	33	14·2
K. C.	355	173	26·5	27	95	28	13·3
D. H.	351	147	36	31	91	24	14·2
G. G.	264	140	31	19	100	43	13·5
E. E.	340	159	40	31	96	46	13·8
G. D.	311	167	34	30	96	38	14·2
K. M.	296	144	30	28	95	38	14·4
D. B.	278	149	28	26	87	31	13·4
I. A.	256	106	26	23	99	26	13·6
Total	3190	1620	307·5	267	940	349	137·2
Av.	319	162	30·75	26·7	94	34·9	13·85
M.V.	38	25·4	3·6	3·16	3·4	6·5	2·7

TABLE II. P. GROUP.

	The tests						
Subjects	Substitution		Opposites	Completion	Memory	Spelling	Age
	A	*B*					
M. S.	360	154	34	27	96	33	12·10
R. W.	350	158	28	25	93	46	13·10
D. L.	335	154	29	28	100	46	13·5
O. F.	328	155	30	27	95	45	13·6
G. S.	305	194	35	20	95	38	13·6
E. C.	389	210	37	21	90	38	13·4
V. H.	307	137	29	28	100	37	13·6
I. C.	296	147	39	31	96	45	13·10
B. M.	268	122	31·5	31	91	30	13·5
M. T.	257	124	34	24	85	42	13·5
Total	3195	1555	326·5	262	941	400	134·7
Av.	319·5	155	32·65	26·2	94·1	40	13·55
M.V.	32·9	19	3·2	3	3·5	4·8	2·1

These tables show that, in ability to perform the tests used, the two groups of children are as nearly as possible equal. The greatest difference is in spelling, and even there it is not very large. In age, too, the groups are nearly identical. If, therefore, one group proves superior in typewriting to the other, this may reasonably be accounted for by supposing that it learnt by the superior method.

The teaching was individual, all the children using the same typewriter. Each subject was given about two half-hours teaching a week, R. (which formed the group taught for speed) on Wednesdays and Fridays, P. (the accuracy group) on Tuesdays and Thursdays. No attempt was made to teach by the touch method; but clearly this does not affect the broad results of the investigation and the problem at issue. The children were allowed to look at the key-board, though they were told that they should try to learn to work without looking. Each lesson began with the typing of the alphabet, after which the subject copied a passage of connected prose (as much as possible in the lesson period) usually taken from a simple translation of the *Odyssey*. The fingering taught was that given in Mrs Clough's book on typing. Gilbreth's pictures of 'champion' typists at work were used to show the proper position of the hands, and the movement of the fingers. This was the same for all. In addition, the speed group were told that of the two essentials for a typist—speed and accuracy—speed is the more important. They were given marks, and now and then prizes, *simply for speed*, and they were never shown their old papers nor were their mistakes mentioned. In the case of the accuracy group the procedure was reversed. They were told to aim at accuracy, were marked on this, and they were *constantly shown their papers with their mistakes corrected*, and nothing was said about speed.

The experiment began on November 2, 1920, and continued with breaks for holidays till May 6, 1921. During this period each child had had about 16 hours' practice. The characters of the performances of the two groups were by this time clearly defined, and as a continuance of the same methods seemed unlikely to produce any important change, the instructions given to the groups were *reversed* in order to see how permanent the results were, and whether the earlier teaching would render the children unable to acquire the characteristic that had been previously neglected. After this change in method, 11 lessons were given to each group, extending from May 10 to June 30, each child receiving about six hours' teaching.

In scoring it was thought better to use key-strokes (letters, spaces, etc.) as the basis rather than words; as the former method was both more accurate and easier to work. The alphabet was typed at the beginning of each lesson. As it contains 51 such strokes, the speed and accuracy of typing the connected prose were calculated on this basis of 51 key-strokes so as to make all records as nearly comparable as possible. The speed given in the table of results below is the time taken to type 51 strokes (the whole time any subject typed at a lesson having been recorded, as also the total number of strokes made and the time for 51 strokes calculated from this); and the accuracy, the number of mistakes made in 51 strokes, was calculated in the same way. Errors in spelling, spacing, capitals, and punctuation were counted as mistakes.

III. Results.

The main results of the investigation are shown in Table III and the curves corresponding to it (Figs. 1–3).

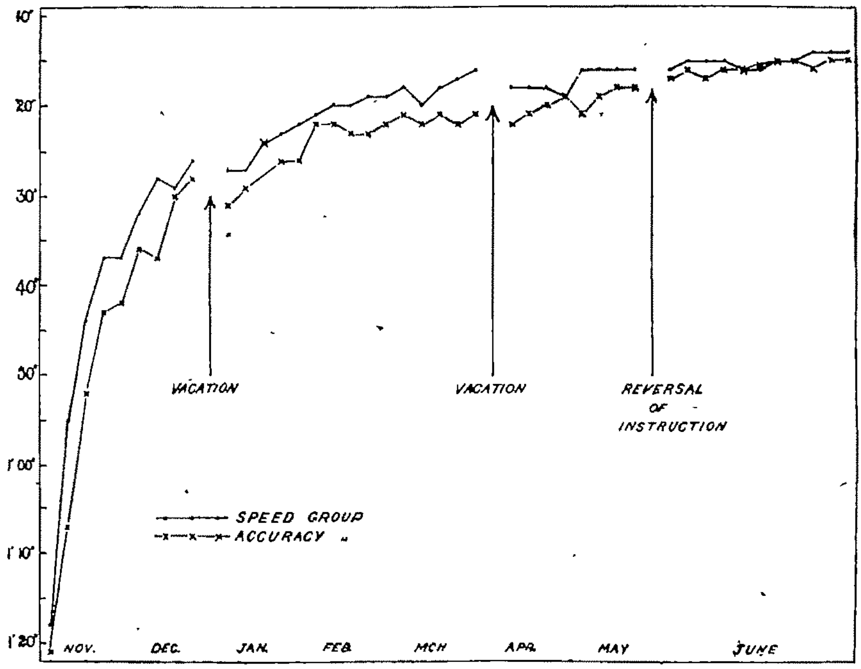

Fig. 1. Average speed of each group in making 51 key-strokes in typing the alphabet (at the beginning of each lesson) at the different stages of the experiment. The higher the curves the better the performance.

The times given in Table III (representing *speed* of work) are the average times taken by the subjects of each group for 51 key-strokes, independently of whether the strokes were correct or incorrect. The two last columns give the average numbers of mistakes (incorrect strokes) made by the subjects of each group per 51 key-strokes.

Let us first consider the typing of the alphabet. It will be seen from Fig. 1 (and the corresponding part of Table III) that the accuracy group takes slightly longer than the speed group to type the alphabet, though

TABLE III.

Date	Time taken to type alphabet (51 key-strokes)		Time per 51 key-strokes taken to type connected prose		Number of mistakes per 51 key-strokes (connected prose)	
	Speed group	Accuracy group	Speed group	Accuracy group	Speed group	Accuracy group
Nov. 9	1′ 18″	1′ 21″	—	—	—	—
„ 11	55″	1′ 7″	1′ 42″	2′ 7″	1·27	1·25
„ 16	44″	52″	1′ 28″	1′ 45″	1·35	1·12
„ 18	37″	43″	1′ 20″	1′ 31″	·95	·75
„ 23	37″	42″	1′ 12″	1′ 34″	·93	·62
„ 25	32″	36″	1′ 7″	1′ 23″	·83	·57
„ 30	28″	37″	1′ 6″	1′ 23″	·89	·52
Dec. 2	29″	30″	1′ 5″	1′ 29″	1·13	·59
„ 8	26″	28″	1′ 4″	1′ 15″	·82	·47
Vacation						
Jan. 13	27″	31″	1′ 1″	1′ 20″	·83	·19
„ 18	27″	29″	58″	1′ 11″	·98	·39
„ 20	24″	24″	59″	1′ 2″	·69	·35
„ 25	23″	26″	54″	1′ 8″	·84	·25
„ 27	22″	26″	53″	1′ 4″	·78	·35
Feb. 1	21″	22″	52″	1′ 5″	·79	·25
„ 3	20″	22″	49″	1′ 11″	·83	·30
„ 8	20″	23″	49″	1′ 11″	1·00	·28
„ 10	19″	23″	46·5″	1′ 1″	·78	·28
„ 15	19″	22″	45″	1′ 4″	·94	·20
„ 17	18″	21″	46″	1′ 3″	1·04	·29
„ 22	20″	22″	43″	1′ 5″	1·07	·24
„ 24	18″	21″	44″	1′ 1″	·92	·28
March 1	17″	22″	42″	58″	1·03	·30
„ 3	16″	21″	39″	1′ 1″	1·05	·15
Vacation						
April 8	18″	22″	43″	58″	1·11	·26
„ 13	18″	21″	43″	56″	1·14	·25
„ 15	18″	20″	42″	59″	·87	·15
„ 20	19″	19″	42″	55″	1·16	·17
„ 22	16″	21″	39″	48″	1·01	·12
„ 29	16″	19″	42″	58″	1·14	·38
May 4	16″	18″	39″	57″	1·44	·25
„ 6	16″	18″	38″	52″	1·13	·20
Reversal of Instructions						
May 13	*16″*	*17″*	*41″*	*44″*	*·63*	*·33*
„ 18	*15″*	*16″*	*44″*	*43″*	*·47*	*·33*
„ 20	*15″*	*17″*	*39″*	*44″*	*·50*	*·18*
„ 27	*15″*	*16″*	*41″*	*40″*	*·49*	*·24*
June 1	*16″*	*16″*	*41″*	*41″*	*·43*	*·29*
„ 8	*16″*	*16·5″*	*38″*	*39″*	*·42*	*·29*
„ 10	*15″*	*15″*	*39″*	*39″*	*·47*	*·25*
„ 15	*15″*	*15″*	*40″*	*39″*	*·41*	*·24*
„ 17	*14″*	*16″*	*42″*	*43″*	*·47*	*·28*
„ 22	*14″*	*15″*	*39″*	*41″*	*·41*	*·32*
„ 24	*14″*	*15″*	*37″*	*37″*	*·33*	*·31*

NOTE TO TABLE III.

The dates in this Table are those for one group only: the true dates for the other group are in each case one day later or earlier than the given dates. But for convenience of tabulation the dates for the two groups are made the same, as this in no way affects the results. As was explained above, the speed group had its lessons on Wednesdays and Fridays, the accuracy group on Tuesdays and Thursdays.

On the first lesson day only the alphabet was typed, the remainder of the lesson being taken up with explanations. This accounts for the blanks in the Table for date November 9.

the difference between them practically disappears after the reversal of instructions. This approximate equality between the groups in typing the alphabet seems to show that, as far as the mechanical part of the typing went, the groups were equal; such difference as was shown during the earlier part of the learning process being due to the difference of methods.

From all the other results, however, it is clear that the two methods of teaching produced very different results. Almost from the beginning the two groups began to draw apart, and this difference became more marked as time went on. For instance, on December 10, when the groups had had ten lessons (amounting to about five hours' practice) the speed group was 17 per cent. superior as regards speed, and the accuracy group was 74 per cent. superior as regards accuracy. These differences increased. By May 4, after 33 lessons or about 16 hours' practice, the speed group was superior in speed by 38 per cent. and the accuracy group in accuracy by 450 per cent.

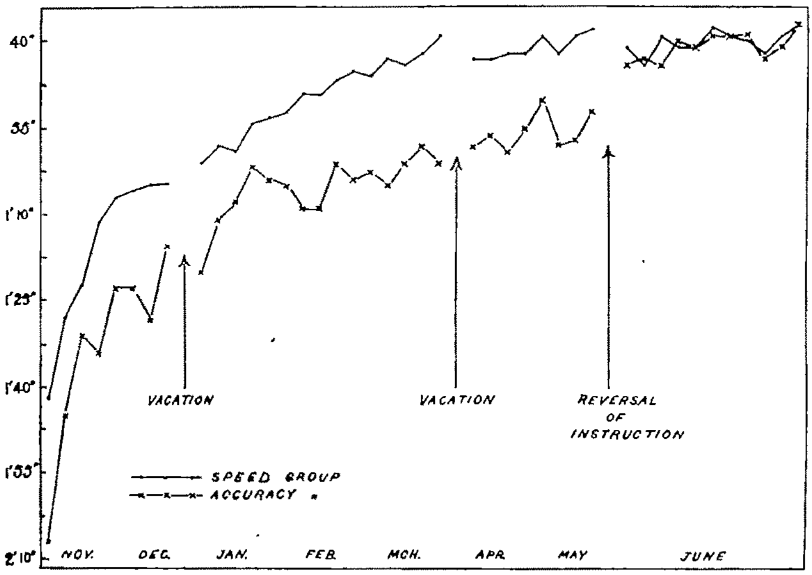

Fig. 2. Average speed of each group per 51 key-strokes of connected prose at the different stages of the experiment. The higher the curves the better the performance.

It was at about this point that the method of teaching was changed. Both groups were now treated alike, and encouraged to attend both to speed and accuracy, though with each group most stress was naturally laid on what had hitherto been neglected. The result, as can be seen, was that the two groups drew practically level, the accuracy of both groups rapidly approximating, and the difference in speed disappearing. The fast group did not, however, attain quite the same level of accuracy as the accuracy group until just at the end of the experiment. The speed of the two groups remained the same throughout this final period. It should be noted particularly that the speed group acquired at once almost the accuracy of the accuracy group *without appreciable loss of speed*, and similarly, that the accuracy group acquired at once the speed of the speed group *without appreciable loss of accuracy* (cf. Figs. 2 and 3).

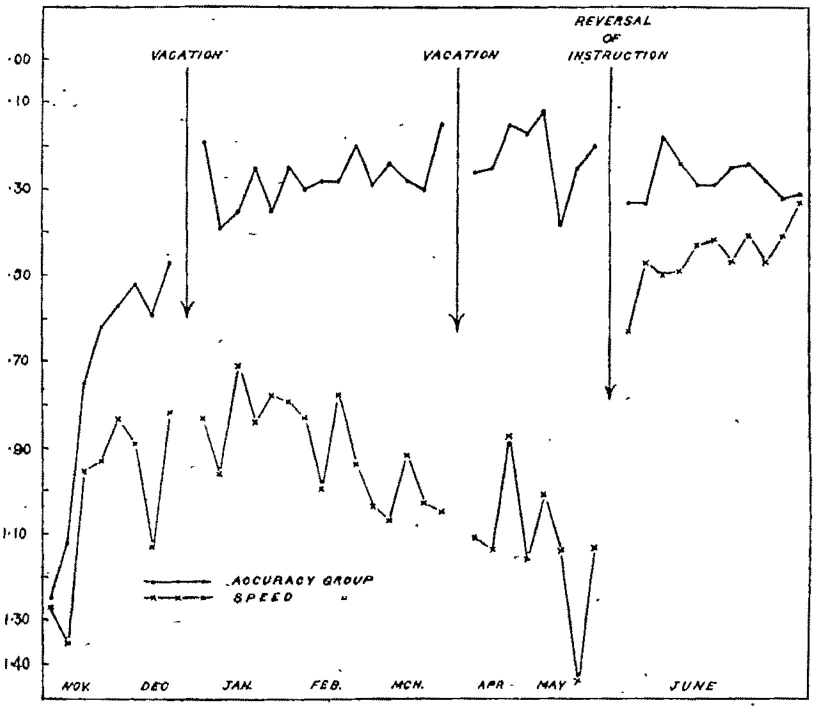

Fig 3. Average number of mistakes (incorrect key-strokes) per 51 key-strokes made by each group in typing connected prose at the different stages of the experiment. The higher the curves the better the performance (the greater the accuracy of the typewriting). Ordinates indicate number of mistakes per 51 key-strokes.

IV. DISCUSSION.

From the foregoing results it is perfectly clear that so far as typewriting is concerned Gilbreth is wrong in his contention that learning motions slowly is no help to the making of them quickly after they are learnt. The rate at which a beginner *is allowed* to work need make no difference whatever to the rate at which he *can* work after a certain amount of practice. When required, children who for six months have been allowed to work as slowly as they like, can acquire the speed of a group that has been exhorted from the beginning to work as fast as possible. The improvement is immediate, and the records of the two groups remain to the end so similar that they may be considered identical.

It is nearly as clear that the talk about the deplorable results of 'careless haste' is equally unfounded. Children who have been making over eighty mistakes in a page of typing, can, in the course of three lessons, reduce them to nine, and that with only the very smallest decrease in speed. But, at the same time, it must be noted that accuracy will not improve of itself as speed will. The group that was trained in accuracy gradually improved in speed: but the group that was trained in speed did not constantly improve in accuracy—on the contrary, after an initial improvement it gradually became less accurate (cf. Fig. 3). To effect an improvement in accuracy it needed a fresh stimulus directed to this particular point; and to the experimenter it seemed that the *rate* of the improvement in accuracy depended mainly on the degree of influence she had over individual children, some responding with a great improvement in the first or second lesson, others, who were noticeably slow and apathetic about the whole of the teaching, only improving gradually. The cases in which carelessness has become a 'habit' are doubtless those in which there has never been any adequate stimulus to accuracy.

There is another point in connexion with teaching method. The curves for accuracy fluctuate, in general, more than the curves for speed. This is suggested by Figs. 2 and 3. Table IV gives exact figures for one section of the curve. Here the scores (for both speed and accuracy) for the eight lessons from April 7 to May 6 have been averaged, and the daily deviations from these eight-day averages calculated as percentage of the eight-day averages. The results are as follows:

During this period, then, the average fluctuations in accuracy are, in the accuracy group, four times, and in the speed group twice, those of speed. This may be due to temporary causes, such as distraction or fatigue, having a greater influence on accuracy than on speed. A method of teaching, in any case, which is based upon insistence on accuracy must involve more constant rebuke and be more unpleasant to the learner than one which makes speed its aim; yet, if the quality of the *immediate* product is the chief concern, this greater variability as regards accuracy renders constant attention to it necessary. If, however, the quality of only the *final* product is the important matter, the period of learning may be made much more pleasant for the pupil (and for the teacher too) by neglecting fluctuations in accuracy, since accuracy by the way seems not to be essential for final accuracy.

TABLE IV.

Showing deviations of the daily group average from the group average for eight days in both speed and accuracy; the deviations being expressed as percentages of the eight-day averages.

	Accuracy group % deviation from average for			Speed group % deviation from average for	
Date	Speed	Accuracy	Date	Speed	Accuracy
April 7	4·7	17·6	April 8	4·9	1·5
„ 12	1·0	10·6	„ 13	4·9	1·2
„ 14	6·5	30·5	„ 15	2·5	22·4
„ 19	0·7	23·7	„ 20	2·5	2·9
„ 21	13·4	45·0	„ 22	4·9	9·9
„ 28	4·7	23·7	„ 29	2·5	1·2
May 3	2·9	11·9	May 4	4·9	28·3
„ 5	6·1	9·7	„ 6	7·3	·3
Total	40	172·7		34·4	67·7
Average	5	21·6		4·3	8·5

Certain features of the experiment illustrate serious difficulties in the way of carrying out Gilbreth's suggestion, even if his general contentions were correct. He assumes that before beginning to teach habits involving (or consisting of) muscular activity, the best motions have been discovered and motion models set up. In typewriting, at all events, owing to the large number of combinations of movements, the latter is impossible, and, from this experiment, it seems unlikely that the same movements are desirable for all individuals; *e.g.* many of the children seemed unable, through weakness, to use the little finger of the left hand. Moreover, many of the children seemed to find great difficulty in learning to make, or to omit to make, some particular movement. In the speed group this difficulty was more noticeable than in the accuracy group, apparently because the effort to work fast prevented them from paying adequate attention to the details of their movements. Near the end of the experiment, notes were taken of the fingering and the position of the hands of the children; of the four who were best, three came from the accuracy group, and of the four worst, three were from the speed group. On the average too, the movements of the accuracy group were considerably more 'correct.' This result is only what might be expected in learning a habit involving varied and difficult movements. Unless some extremely stringent system of discipline is in use, it is impossible, if speed is demanded from the start, to insure absolute correctness of movement. Of course, the value of 'correctness' of movement is uncertain; but it would probably appear in advanced stages of practice.

V. CONCLUSIONS.

1. If movements such as are employed in typewriting are learnt slowly at first, the length of the learning process is not thereby increased; and it appears highly probable that the same speed can be obtained finally as when speed is insisted on from the start.

2. If during learning the attention is directed solely to accuracy, the speed will gradually improve. If attention is directed solely to speed, the accuracy tends to diminish.

3. Between the two methods employed—(*a*) exclusive attention to speed, and (*b*) exclusive attention to accuracy—there seems little to choose when considered simply as methods of learning. The choice must depend on other considerations, *e.g.* on how soon it is desired to use the product.

4. Without special apparatus it seems unwise to demand speed from the *very* beginning, as it becomes more difficult for the learner to acquire 'correct' movements (assuming these to be ultimately valuable).

5. It seems probable that a high quality of work can be obtained finally, without insisting on a high quality constantly throughout the learning process. This would appear to have a very general bearing on methods of teaching.

PUBLICATIONS RECENTLY RECEIVED

Suggestion and Autosuggestion. By CHARLES BAUDOUIN. Translated from the French by EDEN and CEDAR PAUL. London: George Allen and Unwin. 1920. pp. 288. 15*s.* net.

This very entertaining book shows once again that successful practice in psychotherapy has very little to do with whether the practitioner has clearly thought out the theoretical basis of his work. The underlying principle throughout is that every idea tends to find active expression. When the process leading to such expression is subconscious, there is suggestion (p. 26). But 'idea' is never really defined at all, and consequently can be used in all kinds of ways, and with many different shades of significance. We are told that 'idea' is simply equivalent to 'representation'; but this does not help us very clearly to understand what can be meant by such statements as: "The idea of a sensation of pleasure or pain, the idea of a feeling, tends to become this pain, or this feeling." The difficulty is not rendered any easier when we read: "the idea of a sentiment is already the sentiment." It is suggested in one place (pp. 42–3) that an idea is some kind of a *schema*, and in another (p. 78) that it is merely a tendency. In fact, Baudouin's terminology in this respect is extremely muddled. When we turn to other matters there is the same difficulty. Baudouin shows immense enthusiasm, but he mistakes certainty for clearness. What, for example, can be made of the 'law' "In the conflict between the will and the imagination the force of the imagination is *in direct ratio to the square of the will*"? The really interesting thing is how such formulations come to have their undoubted practical value.

The book is always interesting, often acute, sometimes illuminating; but very few readers who are not carried away by enthusiasm will admit that as a theoretical formulation it can be regarded as satisfactory.

The Psychology of Thought and Feeling. By CHARLES PLATT, Ph.D., M.D. London: Kegan Paul, Trench, Trübner & Co. 1921. pp. x + 290. 7*s.* 6*d.* net.

This is best described as a 'nice' book. It contains nothing of importance that is new, but it is written with a good knowledge of modern psychology, and is interesting throughout.

Reports of the Industrial Fatigue Research Board. No. 11, *Preliminary Notes on Atmospheric Conditions in Boot and Shoe Factories.* By W. D. HAMBLY and T. BEDFORD. pp. 69. 3*s.* net. No. 13, *A Statistical Study of Turnover in Munition and other Factories.* By G. M. BROUGHTON, E. M. NEWBOLD and E. C. ALLEN. pp. 92. 3*s.* net.

The first of these two Reports contains nothing of immediate psychological interest; it "deals with the practical use of the Kata-thermometer as an indicator of atmospheric conditions from the physiological aspect," and gives many series of readings, showing how the instrument may be employed to measure the cooling power of the air. The second which includes enquiries into the labour turnover of three factories engaged in making biscuits, leather goods and drugs, will prove of great value to industrial psychologists as an attempt to measure labour wastage by a uniform systematic procedure. Evidence is adduced that the high leaving rate in the early months of factory service was not restricted to munition factories during the war but is still in other factories a source of serious economic loss and is very largely due to such often avoidable causes as ill-health, incompetence and discontentment.

Psychotechnik und Taylor-System. Von K. A. TRAMM. Erster Band. Berlin: J. Springer, 1921. S. vii + 140. M. 24; geb. M. 29.

The author is an engineer, apparently in the service of the Berlin tramways. His descriptions of motion study work apply especially to the training and selection of employees in this occupation. Most of them are elaborations of the methods of Gilbreth. In several instances the author gives no reference to the source of his illustrations; in others the work of Moede, Piorkowski and Amar has been evidently drawn upon. The book will prove of considerable interest and value to engineers and others who are unacquainted with the progress of industrial psychology.

Man's Unconscious Passion. By Dr WILFRED LAY. New York: Dodd, Mead and Co. 1921. pp. 246. 10*s.* 6*d.*

This book deals with the dangerous results of unconscious incestuous passions. The theory is ingenious, but the treatment unscientific in the extreme; no evidence whatever is brought forward to support the statements which are made. Unconscious passion itself is defined in two different ways and no attempt is made to make the two definitions mutually compatible.

The Depths of the Soul. By Dr W. STEKEL. London: Kegan Paul, Trench, Trübner & Co. 1921. pp. 216. 6*s.* 6*d.*

This is a well-translated, readable book, in which the author discusses in popular language a number of human affections and motives which are common in every-day life. The interest of the book consists in the fact that the topics are treated from the standpoint of psycho-analysis.

The Care of the Adolescent Girl. By PHYLLIS BLANCHARD, Ph.D. London: Kegan Paul, Trench, Trübner & Co. 1921. pp. xxi + 201. 7*s.* 6*d.*

This book discusses the special difficulties attending the mental transition from childhood to womanhood in the light of the work of Freud, Jung, and Adler. The cases with which the discussion is illustrated are largely taken from the author's psychological experience in this field. In spite of some vagueness and possible exaggeration of statement at times the book contains a useful summary and illustration of the views and facts with which it deals.

Die differentielle Psychologie: in ihren methodischen Grundlagen. Von Dr WILLIAM STERN. Dritte Auflage. Leipzig: J. A. Barth. 1921. S. x + 546. M. 63; geb. M. 72.

This is a reprint of the edition of 1911 which was exhausted several years ago. It contains, as an *addendum*, an account of the recent progress of the psychology of individual differences and a bibliography of nearly 400 books and papers on the subject which have appeared between the years 1911 and 1921.

Periodic Variations in Efficiency: as shown in mental and physical tests together with some weather effects. By ARCHIBALD G. PEAKS. Baltimore: Warwick & York. 1921. pp. 95.

This is a study of general periodic changes in human life, and especially of the rhythms of growth through which youth passes, while developing into manhood. The author gives a summary of work already done upon the subject; together with a criticism of the means of investigation employed by his forerunners. In the experimental part of his work, the author deals with the question of a seasonal periodicity (i) in muscular strength, and (ii) in mental ability. A prolonged series of tests was made upon schoolboys; the Smedley dynamometer being used to test variations in muscular strength, while for demonstrating any periodic variations in mental ability, a primary memory test was given. The results appear to show that as regards the physical strength of growing boys, there is a period of increase from September to about the middle of December; this is followed by a period of depression between January and March, while a renewed period of growth in strength begins in March and continues till June. The curve for mental efficiency tends to resemble that for physical strength. It shows a rise from September to the end of the year with another increase from March to May, the drop between these periods of increase also appears, but is less marked than is the case of muscular efficiency.

NOTES ON RECENT PERIODICALS

The American Journal of Psychology, Vol. XXXII.

No. 1, January, 1921.

The American Journal of Psychology (G. Stanley Hall and E. B. Titchner).
Gives a history of the Journal on the occasion of Prof. Stanley Hall's retirement from the editorship.

Early Psychological Theories of Herbert Spencer (G. B. Denton).
Discusses Spencer's theories of 'faculties,' 'fatigue' and 'attention,' as given in his *Philosophy of Style.*

The Comparative Influence of Majority and Expert Opinion (H. T. Moore).
Shows by experiments that expert and majority opinion have equal sway over the individual in Morals and Music, but that in matters of linguistic preference the chances are ten to seven in favour of majority prestige.

The Number-Forms of a Blind Subject (R. H. Wheeler and T. D. Cutsforth).
Describes how the blind subject never visualises numbers, but translates colours into numbers in vocal-motor fashion.

Some Problems in Regard to Alimentary Sensitivity (J. G. Campbell).
Hunger is perceptual and appetite involving a different conscious state from hunger. Sensations of fullness are due to stretching of the muscular coat. The stomach certainly and the duodenum probably have thermal Sensibility.

Pleasantness and Unpleasantness in Relation to Organic Response (P. T. Young).
There is no organic-kinæsthetic *sine quâ non* of affection. Unpleasantness is organically positive and active, while pleasantness is negative and passive.

An Experimental Study of Kinæsthetic Imagery (A. H. Sullivan).
Kinæsthetic images of memory are uniform, simple and lacking in body, whereas kinæsthetic sensations are varied and complex.

Affective Tendency as Conditioned by Colour and Form (M. Yokoyama).
As far as relative pleasantness is concerned colour and form are mutually independent in producing affective results, even though they are simultaneously operative.

Brentano and Wundt. Empirical and Experimental Psychology (E. B. Titchner).
A comparative study of the two writers, Brentano's psychology being essentially a matter of argument, and that of Wundt being essentially a matter of description.

A preliminary Study of the Range of Visual Apprehension (S. W. Fernberger).
Shows that the statistical limen, that stimulus value for which 50 per cent. correct judgments are given, is the most reliable measure of visual apprehension.

Emotions and Instincts (H. C. Link).
The notions of 'emotion' and 'instinct' as represented in controversies centering round James-Lange theory are obscure. McDougall's identification of instinctive responses with a "group of emotions arbitrarily selected" is untrue to fact.

The Relation of the Pleasantness of Colour Combination to that of the Colours seen singly (M. F. Washburn, D. Haight and J. Regensburg).
The unpleasantness or pleasantness of a colour combination is derived not merely from summation of the affective tones of its components, but from another factor dependent on the combination itself.

No. 2, April, 1921.

Wilhelm Wundt (E. B. Titchner).

An appreciative review of the career of Wilhelm Wundt.

The Child Mind (H. J. Mulford).

Since mind is the expression of the brain, the development of the child mind is only a development of consciousness in the child brain.

On the Relevancy of Imagery to the Process of Thought (C. Comstock).

If imagery is present as part of the contents of thought, it is *ipso facto* relevant to the thought.

An objective Interpretation of Meanings (J. R. Kantor).

Meanings are definite responses to stimulating objects in their proper setting, differing from differential reactions in that they condition a succeeding act.

A quantitative Investigation of the Effect of Mode of Presentation upon the Process of Learning (F. J. O'Brien).

The modality of imagery which a learner employs is determined by his ideational type, and only secondarily by the mode of presentation.

Voluntary Control of Likes and Dislikes; The Effects of an Attempt Voluntarily to change the Affective value of Colours (M. F. Washburn and S. L. Grosse).

Extreme judgments of preference are harder to change voluntarily than moderate ones and judgments of extreme unpleasantness are decidedly harder to change than judgments of extreme pleasantness.

Journal de Psychologie normale et pathologique (XVIIIe Année).

No. 7, July, 1921.

Doctrines et méthodes psychologiques de l'Inde (P. Masson-Oursel).

The metaphysical assumptions of Indian Psychology are contrasted with those of European Psychology. This is followed by a *resumé* of the essentials of (1) Brahmanistic Psychology of the Vedas and Brahmanas, (2) Buddhist and Jain Psychology and Metaphysics, and (3) Hindu Psychology, religious and philosophical. Finally an account is given of the psychological analysis of the individual which is common to all branches of Indian thought.

Un fait de rire (G.-H. Luquet).

An incident in the war introduces a discussion of laughter as a liberator from painful situations. The kinds of stimuli and situations which produce laughter are distinguished. Special attention is paid to the muscular response to such relieving stimulation.

L'interprétation du rhythme du cœur dans certains rêves (M. Déat).

Three dreams occurring to a subject in a fatigued state present markedly rhythmic characteristics which are ascribed to the rhythm of the heart-beat, changes in the former rhythm being correlated with changes in the rhythm of the latter.

A report of a meeting of the Société de Psychologie is added with papers by P. Sollier on *The Neuro-muscular phenomena which accompany the processes of rapid 'weaning' from morphine*, and by R. Stumper on certain points in the psychology of ants.

Psychological Monographs, Vol. xxx.

No. 1, Whole No. 134.

A Tentative Standardisation of a Hard Opposites Test (Marie Haekl Means).

The aim of the writer was to obtain a standard 'Hard Opposites Test' suitable for application to college students. Stimulus words were selected, and graded in order of difficulty by statistical methods. Each of these words had a number of possible responses, and a response was assigned a value by averaging the values given in each case by five independent judges. A list of these stimulus words in order of difficulty was arranged as a group test, and copies were sent to a large number of colleges and universities. In this way records from 1628 students were obtained, from which norms for college students were established. These standards are presented in terms of percentiles for each class of students, including graduates; and for each sex.

The results of the investigation tend to show that success in the test depends on native ability rather than on years of schooling, since there is a great overlapping among the classes, and also since the lowest scores gradually rise from year to year while the highest scores remain constant.

No. 2, Whole No. 135.

The Influence of the Time Interval on the Rate of Learning in the White Rat (Joseph M. Yarbrough).

The objects of these experiments was threefold:

(1) To investigate the influence of the time interval upon the rate of learning;

(2) To determine the readiness with which a given temporal association will function backwards, and the most favourable time interval for this functioning; and

(3) To consider the claims of the rival theories of simultaneous and successive presentation in relation to the rate of learning.

The experiments were carried out in an improved problem box. The animals were required to learn first a negative response to pain; then to transfer the response to an auditory stimulus introduced in a definite temporal relation to the pain stimulus; and finally, to substitute the response from the auditory to a visual stimulus. The number of trials necessary to learn each problem was recorded.

The results show that the number of trials necessary to learn the association does not increase proportionally with increase in the time interval between the presentation of the pain and auditory stimuli. Also association backwards is very little more difficult than forwards when a continuous mode of presentation is used, but a time interval of one second probably makes backward association impossible. The writer concluded that the simultaneous mode of presentation is slightly superior when the exposure time *of the stimuli* per term is unequal; but if the exposure time is equal the better mode of presentation depends on the material used.

Journal of Experimental Psychology.

No. 1, February, 1921.

The Inspiration-Expiration Ratio During Truth and Falsehood (Harold E. Burtt).

When a person is consciously lying, in general, certain of his emotions are aroused, and the deceit may be indicated by a measure of emotional expression. The inspiration-expiration ratio may be used with fair success as an indication of deceit, but systolic blood pressure forms a better criterion. Both measures may be to some extent vitiated by voluntary control.

An Experimental Study of Motor Ability (F. A. C. Perrin).

Even the simplest motor test brings into play a variety of factors which ordinary experimental conditions do not attempt to analyse or isolate. In particular there can be no satisfactory study of motor accomplishment apart from a consideration of how motives operate.

Comparative Simple Reactions to Light and Sound (F. L. Wells, C. M. Kelley, Gardner Murphy).

The relation between reaction time to sound and light is dependent on the magnitude of the stimuli compared, and in addition may differ very considerably for different individuals. There is greater individual difference in reaction time to sound than to light.

Experimental Study of the Stimulus Error (Samuel W. Fernberger).

The different results obtained from the well-known experiments according as the subject's attention is directed upon the character of the stimulus or upon an analysis of his own responses, are set forth, and the significance of both attitudes discussed. It is claimed that the experiment shows the futility of a statistical treatment of a purely behaviouristic study, apart from introspective control.

Zeitschrift für Psychologie. Band 87.

Heft 1 und 2. Juni, 1921.

W. Köhlers physikalische Theorie der physiologischen Vorgänge die der Gestaltwahrnehmung zugrunde liegen (Erich Becher).

An account and criticism of W. Köhler's theory of Form. Köhler has attempted to account for the fact that the perception of a whole is other than the mere sum of the perceptions of its parts. He deals first with a physical theory of form, and secondly with the physiological events concomitant with the perception of form in audition, vision, and taste. He then proceeds to establish a relation between the first and the second.

Studien zur zentralen Transformation der Farben (Georg Marzynski).

When shadows are cast on a white ground the patch thus formed is matched with grey, and yet the white ground shows through the shadow in spite of the fact that the same amount of light stimulation can be shown to be operative in both cases. The possible objective and subjective causes of this are dealt with. Kindred problems of the relation between shadowed and dimly-lighted patches, and the matching of shadows are noticed. The bearing of Weber's law on the problem is particularly considered, and an account is given of some shadow experiments.

Über die Vorstellungswelt der Jugendlichen und den Aufbau des intellektuellen Lebens (E. R. Jaensch).

The author has edited a collection of papers on the objective world of children and the building up of the intellectual life, of which three are here presented by Edith Gottheil, E. R. and W. Jaensch, and Alfred Jösser.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETING

October 15, 1921.	The Origin of Affective Tone, by C. S. Myers.
December 10, 1921.	Tests of Practical Ability, by Margaret M^cFarlane.

SECTIONAL MEETINGS.

Education Section.

October 4, 1921.	A Basis for Experiments in Æsthetic Appreciation, by Margaret Bulley.
November 8, 1921.	The Problems of Intelligence and Intelligence Tests, by J. A. Green.

Medical Section.

October 26, 1921.	The Affect in Dreams, by W. H. R. Rivers.
November 23, 1921.	The Castration Complex, by Douglas Bryan.

VOLUME XII APRIL, 1922 PART 4

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THE DEVELOPMENT OF THE KNOWLEDGE OF TIME IN CHILDREN.

BY E. C. OAKDEN AND MARY STURT.

§ 1. *Introduction and tests employed* (pp. 310, 311).
§ 2. *The 'questions' test* (pp. 311–316).
§ 3. *The 'order of date' test* (pp. 316–318).
§ 4. *The 'temporal absurdities' test* (pp. 318–321).
§ 5. *The 'temporal completion' test* (pp. 321–323).
§ 6. *The 'pictorial identification' test* (pp. 323–327).
§ 7. *The 'temporal order of historical characters' test* (pp. 327–329).
§ 8. *The 'temporal memory' test* (pp. 329–333).
§ 9. *The 'making an appointment' test* (p. 333).
§ 10. *Summary of experimental results* (p. 334).
§ 11 *General educational implications of the tests* (pp. 335, 336).

PREVIOUS investigations in 'time-sense' have been concerned mainly with ability to estimate and reproduce intervals of time; this reproduction being made without reference to the conventional scheme of time measurement expressed by our system of minutes, seconds, etc. Such investigations were intended to throw light on the origins and primitive forms of the time experience. In the following experiments, on the other hand, our aim has been to inquire into the knowledge and appreciation of time as measured by instruments of precision and arranged in a conventional scheme. Knowledge of this 'conventional time,' as possessed by the educated adult, is the result of a gradual process of learning—not due to some innate faculty or acquired suddenly in the course of infancy or childhood. The learning proceeds partly by contact with adults, who already know the use of the different time-words, and partly through definite teaching in such subjects as history. Indeed, the comprehension of time as measured by conventional means is a condition of the full appreciation of many school subjects, especially those with any historical elements. Therefore, our aim in the following investigations into children's knowledge of 'conventional time' is mainly educational.

§ 1. *Introduction and tests employed.*

The growing sense of time in children has been studied through their vocabulary—a method which, though probably the best yet tried, suffers from the disadvantage (especially in the youngest children) that the inability to use a word correctly does not always mean that the experience connoted by the adult use of the word is lacking. The subjects of our own experiments were chosen from the schools; they do not therefore include any children under four years of age. This objection has therefore less weight in the present case. What we have attempted to trace in these children is the growth of that sense of time which gives us the power to think in dates, to make and keep appointments, and to form plans involving temporal factors. It is obvious that in making such plans adults, at least, think directly in conventional units of time, *e.g.* minutes and seconds, and not in subjective sensations of duration.

On these problems little work seems to have been done; yet children's ignorance and incapacity in such matters are notorious. Binet's observations on the subject are well known[1]. In his 1906 scale he ascribed to children of 6 years the knowledge whether it is morning or afternoon, and to those of 9 years the knowledge of the day of the week, the month, day of month, and year. At 10 he supposes children to be able to repeat the names of the months correctly. In his study of reasoning in children[2], Burt comments on the great difficulty they seem to find in making "time syntheses," but he does not consider the matter further. The other studies of children's knowledge of time, *e.g.* those of Decroly and Degan, deal with children under the age of 5.

Our own aim being, as we have said, primarily educational, we have attempted to provide experimental evidence bearing on (*a*) the methods of history teaching, and (*b*) the age at which certain aspects of it could most profitably be begun. This seemed to involve an investigation into

(i) the child's understanding of ordinary time-words and symbols such as are in use in every-day life;

(ii) his power to form the conception of a universal time scheme extending into the past and future, and his ability to use the dates by which such a scheme is symbolized;

(iii) his knowledge of the characteristics of definite epochs in the time scheme and his ability to place these epochs roughly in the correct order;

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[1] *Development of Intelligence in Children.* Eng. Trans. 1916, pp. 206–217.

[2] *J. of Exp. Ped.* Dec. 1919.

(iv) the matter and methods used by the child in thinking about historical data;

(v) the importance attached by children to time in comparison with other elements in their experience.

The knowledge tested under the first two heads is clearly an indispensable preliminary to any profitable study of history, if history is to mean more than the mere description of ways of life without reference to chronology or causation. The third and fourth heads rather concern what has been assimilated, and have more bearing on the methods that can be employed in teaching than on the general question of the growth of the concept of time. The last head directly concerns selection and emphasis of material for teaching at different stages.

The tests used were:

A. A list of questions intended to illustrate (i) and (ii) above.
B. Lists of historical characters with dates, dealing with (ii) above.
C. An absurdity test, two completion tests, and the arrangement of people in order, in their centuries and in epochs. The last of these tests had two forms: (*a*) the names of people were given; (*b*) pictures were shown. All these were used to illustrate (iii) and (iv) above.
D. A memory test and the composition of a letter, dealing with (v) above.

§ 2. *The 'questions' test.*

This test was always given individually and orally when given to the younger children. To one set of older children, ages 8–10, it was given in class as a group test, and the answers were written.

The following is a list of the questions asked:

1. What is your age?
2. What is the date of your birthday, (*a*) month, (*b*) day of month?
3. Is it morning or afternoon? Why?
4. What day of the week is it?
5. How long would it take you to walk round this room?
6. What season is it? Why?
7. What month is it?
8. What day of the month is it?
9. What year is it?
10. In what year were you born?
11. What day is it at [*e.g.* Newmarket] now?
12. What time is it now?
13. What time is it for your mother at home?
14. Robin Hood lived in 1187. (*a*) Would your mother be alive then? (*b*) Would your grandmother?
15. Would Christ be alive then?

16. How long is it since the [*e.g.* Easter] holidays?
17. What time does School begin?
18. What time does School end?
19. Shall you come to School on Saturday?
20. How long will it be till holidays begin again?
21. (*a*) When you are [*e.g.* 10], shall you be older or younger? (*b*) Will your mother be older, younger, or the same?
22. How long have you been talking to me?

In answer to Q. 5 two estimates were required, the second (called 5*b*) after the child had actually walked round the room. Q. 13 was always introduced by—"Has your mother a clock at home?" and "Is it right?" It was actually asked in the form, "What time would she see if she looked at it now?" Q. 14 was asked thus, "Have you heard of Robin Hood?" If the answer was "No," the experimenter said, "He was a robber and lived in the forests. He lived in the year 1187. Was you mother alive then?"

In marking, four symbols were used. R = right; W = wrong; – = no answer; ! = an absurd answer. The marking of most of the questions presented no difficulty, but a few questions need special mention.

Qs. 8 and 9 had to be quite right. The children were told the date in school.

Q. 12. Five or ten minutes' latitude was allowed.

Qs. 16 and 20. Estimates of more than a week out counted as wrong.

Q. 22. An answer of five or ten minutes counted right (the tests actually took about 7 minutes); a greater or less time, *e.g.* 2 or 15 minutes, was counted wrong.

Such answers as the following were counted as 'absurd':

Q. 5. Five minutes or over.

Q. 6. Any answer that was quite wrong, *e.g.* "Nearly July, because the apples are on the trees." Any answer that was reasonable, *e.g.* "Summer, because it is so hot" (the day being a particularly hot one in March) was counted as right.

Qs. 7 and 9. Any answer apparently senseless, *e.g.* 3.

Q. 12. A time more than two hours out, or some repeated phrase that seemed to mean nothing.

Qs. 16 and 20. All periods much too long or too short, *e.g.* 5 days instead of about 2 months, or 6 or 12 months instead of 2 months.

Qs. 17 and 18. Any time more than one hour out.

Q. 22. Any time more than half-an-hour out.

The following Table shows the percentage of children answering each question rightly (R), absurdly (!), and failing to answer (–), at each age.

The percentage of children answering wrongly (W) is not shown, but can readily be calculated. The questions are arranged in order of increasing difficulty.

TABLE I.

Age	4			5			6			7			8			9			10		
No. of children	14			12			15			13			25			25			6		
% answering	R	!	–	R	!	–	R	!	–	R	!	–	R	!	–	R	!	–	R	!	–
Number and nature of question																					
1. Age	100	.	.	100	.	.	100	.	.	100	.	.	100	.	.	100	.	.	100	.	.
19. School on Saturday ...	78	.	.	91	.	.	100	.	.	100	.	.	96	.	.	100	.	.	100	.	.
3. Morning	67	.	7	83	.	.	100	.	.	100	.	.	100	.	.	100	.	.	100	.	.
22. When 10 you older ...	50	43	.	75	.	.	80	.	.	100	.	.	92	.	.	100	.	.	80	.	.
2. Birthday month	50	.	29	50	.	50	73	.	20	100	.	.	96	.	.	100	.	.	100	.	.
4. Day of week	21	.	50	73	.	9	87	.	7	92	.	8	97	.	.	88	.	.	100	.	.
14. R. H.: mother	50	.	.	66	.	.	67	.	13	77	.	.	92	.	.	92	.	.	100	.	.
13. Time for mother	50	.	29	75	.	.	60	.	7	84	.	8	84	.	.	92	.	.	100	.	.
21. When 10 mother older ...	14	43	21	42	.	.	87	.	.	84	.	.	88	.	.	92	.	.	67	.	.
18. What time return from school	7	35	48	33	25	16	54	.	15	82	.	.	91	.	.	100	.	.	100	.	.
2. Birthday: day of month ...	7	.	86	25	.	75	27	.	60	69	.	31	96	.	.	100	.	.	100	.	.
11. Day in other town	14	.	21	58	.	.	60	.	.	54	.	16	64	.	7	84	.	8	86	.	.
7. Month	7	7	78	8	.	66	20	13	33	77	.	27	83	4	8	100	.	.	100	.	.
6. Season	7	.	78	50	.	42	31	8	54	27	9	53	83	.	8	96	.		100	.	.
14. R. H.: grandmother ...	35	.	7	58	.	.	40	.	7	38	.	8	63	.	12	89	.		67	.	.
6. Season. Why	14	.	78	63	.	35	30	10	40	9	.	64	74	.	.	82	.	4	100	.	.
17. Time school begins	7	43	29	16	25	16	46	.	7	45	.	.	53	.	.	96	.	.	83	.	.
9. Year	.	14	86	.	8	91	7	13	80	46	.	46	84	7	4	92	.	4	100	.	.
15. R. H.: Jesus Christ	14	.	7	33	.	16	54	.	7	54	.	8	28	.	4	12	.	.	30	.	.
8. Day of month	.	7	93	.	.	66	7	.	73	31	.	54	48	.	8	80	.	.	57	.	.
16. How long since holidays ...	14	7	71	8	33	25	27	27	13	31	.	16	34	14	4	36	8	8	23	.	14
22. Time been talking	.	7	78	25	33	.	20	7	13	46	.	8	27	3	.	24	8	.	29	.	.
20. How long till holidays ...	.	14	64	.	16	33	31	15	31	.	16	38	30	6	8	36	8	.	71	.	.
10. When born	.	7	93	.	.	100	.	.	100	18	.	82	47	6	13	56	12	.	43	.	.
12. Time now	.	35	64	8	33	16	33	7	20	38	.	46	38	.	10	12	.	.	14	.	.
5. How long to walk round (*a*)	.	.	86	.	33	8	.	27	7	.	.	.	22	7	.	8	12	.	14	.	.
5. " " (*b*)	.	21	64	.	12	.	7	27	40	.	8	.	10	13	.	4	16	.	.	14	.

In estimating the ages at which the various questions can be answered, it is perhaps best to consider the columns giving absurdities and failure to answer. The 4-year-old children know very little, and their absurdities are mainly in answers to those questions which involve giving the time of day. At 5 years of age the greater number of absurdities is to be found in the answers to questions on duration; while from 7 onwards the absurdities have largely disappeared, thus showing, on our system of marking, a certain general knowledge of the terms involved. The 10-year-old group is small and came from a class in which the average age was 9; it therefore consisted of children below the average intelligence of the class. In spite of this, the 10-year-old group is on the whole better than the 9-year-old group, and shows a fairly close approximation to the adult knowledge of time.

There seems to be a slight change in the order of difficulty of the questions at different ages. This may well be due to the smallness of the groups investigated. Accidental factors have undoubtedly affected the position of certain questions[1].

To arrange the questions in order of difficulty, and to determine thus the order of the growth of knowledge, are difficult. Some facts, however, seem clear. The duration questions are clearly the hardest, so much so that they are beyond the power of most adults to answer correctly. *E.g.* Q. 5 was only answered correctly by one adult out of seven tested, and he was an amateur photographer practised in estimating short periods of time. The difficulty of Qs. 16 and 20 for the children seemed to be partly due to their inability to perform the necessary calculations, such as would be made by an adult if he could not tell 'off-hand' the length of time before a certain event. It is noteworthy that only one of the younger children made use of organized experience, saying, "I suppose the term will be three months; it usually is."

Next in degree of difficulty may be put the questions involving the time of day. Q. 12 is probably too hard for most adults; the man who can tell the time correctly without looking at his watch being generally regarded as something rather out of the common. It is noteworthy that Q. 17 proves to be much harder than Q. 18. It might be thought that this is due to the children tending to confuse the time of leaving home with that of the actual beginning of school, but for the fact that they give a time that is late quite as often as one that is early. Knowledge of other points seems to grow irregularly; though the tendency is for growth to occur outwards from the more to the less personally interesting, and from frequently recurring cycles to those of longer periodicity.

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[1] Cf. Q. 15; the children of the school which wrote the answers to these questions had clearly been taught that Jesus lives *eternally*, wherefore in almost all cases they said that He was alive at the same time as Robin Hood.

Both the day of the month and the year involve counting and this does not become easy and habitual until a child is about 7 or 8 years old[1]. The season seems to children much less a mark of time than a description of concrete material things that enter directly into his experience. Winter really means for them snow or Spring, flowers. Consequently a wrong season may be given with the right description; on a very warm March day Q. 6 may produce many answers such as, "Summer, because it is so hot." This also explains the greater difficulty children find in giving a reason for their thinking it is morning or afternoon. When a reason was given, and not merely the intuitionist's "I just know," it usually concerned personal activity, often a meal. Such answers as "The sun is setting," occur but rarely.

Perhaps the most interesting results are those bearing on the universal nature of time. Most children realise quite early that time is the same all over the same town (Q. 13 can be answered by 50 per cent. of the 4-year-old children) and also that the sequence of time in the future for others is the same as for themselves (*v.* Q. 21). But even at 10 years of age the fact that time is (practically) the same in different English towns is not realised in all cases. One 8-year-old who knew that it was Saturday in Cambridge, summed up the matter quite finally, "It would be Sunday in London, 'cos it's a different town, and there are towns where it would be Monday." She was pressed for an example. "It would be Monday in Hunstanton, 'cos that is different again." Of course, children seldom express themselves as definitely as this, but in cases where it has been possible to get the child to give a reason at all for his answer, he seems to think as did the child quoted. A difficulty may arise for him from his observation of the fact that different people's time tables vary greatly; 'baby' goes to bed at a time different from 'sister' or 'mother.' Even when 'time' is thought of as being the same within the child's circle of experience, why should it be so in places which are apparently so entirely disconnected from it?

Binet commented on the fact that children found great difficulty in naming the year, a task which appears easy to adults. This knowledge seems to be acquired suddenly, and in a way rather different from that of the other elements of the date. From 4–6 years of age there is practically no knowledge of the year. At 7 about half know, and from 8–10 nearly all. It was, however, far from certain that, because the children could give the year as part of the date, they had any real understanding of a chronological system. When they were asked to give the year of their birth the highest percentage of successful answers was 56 at age 9; but at that age and still more at 10, the children ought to have been able to make the needed calculation if they had understood the principle on which dates were arranged. The other question, 14, intended to test an appreciation of dates, did not seem to give results at all conclusive. It appeared to be Robin Hood's name, rather than the date, that suggested a period remote from ordinary experience. The description given of him as a robber, and the stories of him in the books where he was often described as living in 'the olden times,' probably often determined the right answer when it was given. If the children had made any calculation, even the simplest, there would not have been so much difference between the answers in the cases of their mother and grandmother (Q. 14 *a* and *b*). Apparently, grandmother's experience could be so remote that anything might have happened in those days. One quite clever and well taught child of 8½, who answered practically all the questions rightly, was quite unable to decide about her grandmother. "You see," she said, "Granny lived to be 80." She never made any attempt to obtain an answer from calculation. Another child, aged 6, placed Robin Hood exactly a generation back. "Mother was a baby in his time like I am now, and Granny was a young lady, like Mother is." Many other children said that he lived at a time different from Christ, but when asked whether it was before or after, they replied, "before."

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[1] Ballard, *Mental Tests*, pp. 55, 62, 71.

§ 3. *The 'order of dates' test.*

The 'questions' test showed that children make very little use of dates. This might be due to inability to perform the necessary calculations, or to ignorance of the significance of the dates themselves. We therefore gave the following test in various schools, viz. the two lowest forms of a Girls' High School, X, a Boys' Preparatory School, Y, and two Elementary Schools, A and B, the latter in a slum district. We must thank all these schools for their kindness in allowing us to carry out this and other tests.

On the black board was written:

Attila lived in Hungary in 438 A.D.

Philip lived in Spain in 1585 A.D. (or *Dante lived in Florence in* 1312 A.D.).

Nero lived in Rome in 50 A.D.

This was read out to the children and they were told to write down the names of the people in the order in which they lived, "beginning with the one who lived longest ago—furthest away back in history." The test was performed slowly and additional explanations were given if they were asked for. When the children had done this, a similar test was given containing the names of Plato, Burke and Swift. In its earliest form the test contained five names; but as only 20 per cent. of the children tested, aged 10, were capable of answering, we thought that the number should be reduced to three. The results given are for the shorter form shown above.

TABLE II.

	Attila series		*Burke* series	
Age	No. of children	% answering correctly	No. of children	% answering correctly
8	45	51	45	69
9	75	61	56	71
10	57	67	40	93
11	52	89	40	93
12	40	85	37	92
13	28	96	25	100

Ability to perform the test varied considerably at the different schools. School Y was slightly superior at all ages, though the children were rather hurried; B was much the worst.

The higher scores in the second test are mainly due to the fact that school B did not take it; but as all the schools show a slight improvement in it, a few of the errors in the first test may be due to the unfamiliarity of the task. The majority of the errors, however, show real ignorance. They were mainly inversions; and when reasons were demanded, the most usual reply was, "Philip comes first because his number is bigger." This error was, however, complicated by the fact that some children did not know when the number *was* bigger, *e.g.* in the problem below 1898 was often thought to be bigger than 1901. Indeed some children seemed to judge of the value of a date by its last figure[1]. Other mistakes arose through ignorance of 'B.C.' one child thinking that it came after 'A.D.' but on the whole there were fewer mistakes due to this than might have been expected[2]. The utter confusion of a few of the children can be gathered from the following answers to questions. M.D., aged 10, school A: "Nero lived 50 years ago, Dante 1312, therefore Dante lived first." M.E., 9, A: "Look at Dante and Nero, whose date is later?"

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[1] This is partly explained by the fact that this school adopts the modern idea of dealing with *small* numbers in arithmetical calculations. Numbers greater than 1000 are therefore unfamiliar.

[2] Cf. 'pictorial identification' test later (p. 323).

"Dante's." "Who lived longest ago then?" "Dante." P.M., 10, A: "Why do you think Attila lived after Philip?" "I just guessed, I had nothing to guide me."

The majority of children seemed vague as to the point from which years are enumerated; and even where 'B.C.' was understood, did not realise whether this point is in the future or past.

It was suggested by one teacher that the children failed because of the unfamiliar names and the remoteness of the dates. We therefore gave a problem to some of the same children which was free from these objections. It ran:

> John was born in 1898, Mary was born in 1901. Who is the older?

The results of this test were better than those of the other. With the same children they were:

Age	% answering correctly the *Attila-Burke* series	% answering correctly the *John-Mary* problem
8	48	67
9	57	70
10	50	71

If this difference is due to the cause suggested, it would be another example of the difficulty that children find in generalising their time-knowledge.

§ 4. *The 'temporal absurdities' test.*

The two tests above described show that only from about 9 years of age can the majority of children be assumed to possess a knowledge of the conventional scheme of time-marking used in every-day life and in history. Even at that age it is quite unsafe to assume that much more than a half of the class understands the principle on which chronology is based, or what is intended to be conveyed when the date of some historical character is given.

But dates are merely the scaffolding of history, and an understanding of them is quite distinct from a knowledge of the characteristics of the different periods which they denote. It seems probable that the power to understand this depends partly on age, even apart from the effects of different methods of teaching. We were told by an experienced teacher that she had given up telling stories about the remote historic past to children of 7 because they seemed quite unable to imagine a state of things when there were no trains or when the servant's 'night out' did not have to be considered. At 8 she found that they could manage this.

Given this power to think of the past as different from the present, there still seem to be at least two stages in the development of historical knowledge. The first may be called the 'negative' stage; at this stage the present is distinguished from the past by the qualities which the latter *lacks* rather than those which it possesses. Each child sums up the *positive* characteristics of the past, as it appears to him, in some short formula such as wearing sandals and skins or worshipping idols. He relegates whatever seems to him to be beyond his own experience, or the possible experience of those with whom he comes into close contact, to the dim regions of 'savagery.'

The second stage occurs only when the child begins to distinguish historical periods, and to form a picture of *successive* epochs approximating to that formed by an adult.

The following absurdity test was devised to examine the child's power of distinguishing a historical epoch from the present; and also his power of using effectively such knowledge as was investigated by the 'questions' and 'order of date' tests.

In 55 B.C. Julius Caesar arrived with his troops at Dover. They had had a stormy
crossing, and now, as the sun was sinking, they splashed through the surf to the land.
A
The Britons on the shore were singing at their mid-day meal, and did not notice the
noise the Romans made until the enemy forces were only a quarter of a mile away.
B
A day's march brought the Britons to the spot. The next day, Wednesday, the
C
30th February, the Romans caught a British prisoner. He told them the British
D
troops were discontented as they wished to go home at once to reap their autumn
harvest. Caesar was greatly encouraged and forced a battle the next day. At dawn
E
he offered a sacrifice. Taking off his top-hat he stood before the Altar and prayed
F G
"O, Lord Jesus, may this day, Friday, by others regarded as unlucky, prove fortunate
H
to us." The Romans won a great victory due to their superiority in gun fire. The
British chieftain was taken prisoner and shown in Caesar's triumphal procession
K L
three years later, 58 B.C. His grandson is still living in a remote corner of Scotland.

This test was given with the instructions: "Here is a story. In it are many things which are wrong, nonsense or absurd. You are to underline all the things that you can find wrong or absurd, and to write in the margin why you think them so." An example was then given[1]. The actual test was given to school Y (cf. p. 316) throughout[1], to two forms of school A, ages 8–11, and to forms I, II and III of a Bristol Central School, ages 11–14. The results may thus be tabulated:

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[1] It was taken from the absurdity test given in Ballard, *Mental Tests*, p. 38.

Percentage of children scoring each point.

Age	8	9	10	11	12	13	14		
No. of children	26	51	71	55	65	66	24	Total	Average
A	27	47	49	78	86	79	88	454	65
B	8	18	30	60	66	48	75	305	44
C	35	25	8	33	37	29	21	188	27
D	27	33	41	64	66	76	50	357	51
E	50	49	54	73	83	86	83	478	67
F	15	16	25	44	48	39	58	245	35
G	23	35	34	54	48	53	46	293	42
H	31	37	59	80	95	94	100	496	71
K	27	20	20	42	43	38	12	202	29
L	42	47	61	89	89	83	83	494	70
Average	28·5	32·7	38·1	61·7	66·1	62·5	61·6		

If one considers the results obtained by children at different ages the most noticeable fact is the sharp improvement between the ages of 10 and 11. Both above and below that age there is but little improvement; at that point the score practically doubles. The results of school Y are markedly superior to those of the other schools, the average score in this case for all ages being 56 per cent., and in all the other schools together being 42·1 per cent. In spite of this, however, the order of difficulty of the points is practically identical. This shows that the test presented the same characteristic difficulties to each group, although the boys of school Y were more competent to deal with them.

If we range the points in their order of difficulty, the easiest first, they run:

H Gun fire.
L Grandson still alive.
E Caesar's top-hat.
A Sun-set and mid-day meal.
D Autumn harvest.
B Day's march.
G Friday following Wednesday.
F Christ worshipped in B.C.
K 58 B.C. 3 years after 55 B.C.
C 30th of February.

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[1] It was impossible to test the top mathematical form at Y. This meant that only two of the top classical form did the tests. As the school is predominantly classical the cleverer 13-year-olds and some of the best 12-year-olds did not do the tests. The absence of these boys influences the results of the 'J. C.' test and 'completion' test.

We can divide these points roughly into three classes. H, L, E emphasize the distinction between the past and the present, and are the easiest. A, D, B come next and have to do with time as measured by reference to natural phenomena or personal activity. The last four points, G, F, K, C, exemplify purely conventional marks of time. If we compare these results with those obtained from the 'questions' test (pp. 311–316), the correspondence is, on the whole, remarkably close. In the 'questions' test the 'day of the month' is one of the hardest—almost the hardest if we exclude the very difficult 'duration' questions; while in the above absurdity test C proves the hardest point. In both tests, points relating to the year and to the connexion of Christ with the beginning of the A.D. period occur next in order of difficulty. Points requiring a knowledge of the different seasons and the power to realise, however vaguely, when a period passes the span of life of a grandfather or grandson, came third in order of difficulty. All this seems to show that there is actually a definite order in which these elements of knowledge are acquired, and that this order can be demonstrated in different types of material.

While this test thus confirms the results of the 'questions' test, it also supports the view that the earliest distinction the child makes, is between the present and a historical past which is as different as possible from that present. This was shown to some extent by the reasons given by the children for marking the various points in the absurdity test. In addition to those points which the experimenter intended to be observed, many others were marked as absurd that were not meant to appear so, *e.g.* dating by our calendar. The comments made show a gradual change in type. Those of the younger children almost always run: "They did not have dates." "The Britons had no troops." The past appears to be considered simply as negating existing practices. As the children grow older, a positive standard of judgment appears. "Caesar had a helmet and therefore could not wear a top hat." Or, "They did not know about dates and days till the English came." In one case a boy gave a long and quite correct account of the Roman calendar. This change is noteworthy, and seems to occur mainly about 11 years of age.

§ 5. *The 'temporal completion' test.*

To investigate further the positive differentiation of epochs we used at first two 'completion' tests. The children were given sentences dealing with the dress and social life of the Romans and of the English in the time of Charles I, and were asked to complete the sentences correctly. The marking was necessarily difficult, but it was all carried out by one person and every effort was made to keep it uniform. The papers were marked in five grades G (good), F +, F (fair), F −, B (bad). To be B the completion had to bear no reference to the period indicated, *e.g.* attributing to the reign of Charles I the mud huts and human sacrifices of the ancient Britons. F showed a passable, though generally vague knowledge of the period, G a really definite one.

The following is one of the blanks used. The other was exactly the same, save that it dealt with the Romans.

1. The occupations of the English in the time of Charles I were ....
....
2. Their food consisted of ....
3. They lived in houses made of..
4. Their chief city was ....
5. When they went to war the weapons they used were ....
....
6. When they were at peace they wore on their bodies ....
7. On their heads they wore ....
8. On their feet they wore ....
9. They worshipped...............and to please him they ....
....
10. The language they spoke was ....
11. We know about them because they have left ....
and ....

The scores for school Y were as follows:

I. Romans.

		% at each age scoring		
Age	No. of children	F and over*	G	B
8	14	29	0	43
9	21	33	0	57
10	30	44	3	20
11	40	62	22	25
12	34	71	18	6
13	25	68	20	20

II. Charles I.

		% at each age scoring		
Age	No. of children	F and over*	G	B
8	14	36	0	43
9	20	25	0	55
10	33	42	9	39
11	41	56	17	15
12	36	61	17	17
13	25	76	24	12

* These figures include those scoring F, F +, and G.

The papers were on the whole good. The following is one of the best. It is from the 10-year-old son of a University historian in school Y.

1. The occupations of the English were indulging in village sports, and drinking, singing and merry-making.
2. They lived in large country houses and their tenants in smaller cottages.
3. Their food consisted of eggs, ale and butter.
4. Their chief city was London, but during the civil war, Oxford.
5. In war their weapons were swords and blunderbusses.
6. At peace they wore velvet jackets and trousers, with lace frills.
7. On their heads they wore cocked hats.
8. On their feet they wore top boots or velvet lace slippers.
9. They worshipped Christ and to please him they had church on Sunday.
10. Their language was Old English.
11. We know about them because they have left records of births, deaths and marriages.

The papers marked G were about up to this standard. The same test was given at school A to children aged 10–11. The results were surprisingly bad. No one scored G, the greatest number getting F or F – on the Romans, and B on the English. The reason for this was that the answers were in a large measure identical in the two cases, and the 'sandals' and 'idols' fitted the Romans rather better than the English. The tests were separated by a week's interval, and the difference in period was carefully pointed out. The complete failure of these children in the Charles I test is all the more remarkable as they had been receiving lessons on the social life of England in the time of the Stuarts during the whole term. Practically all the children seemed to have 'lumped' all historical periods together, giving them the general attributes of savagery. We could not be certain, however, whether the children failed in the test through absolute ignorance of the period, or only through an inability to revive their knowledge definitely enough to put it down in writing. The next test was directed to clearing up this point.

§ 6. *The 'pictorial identification' test.*

This test, given in school A to standards II, III, IV and V, was intended to show whether the children could recognize and distinguish different historical epochs, and, if they could, what were the indications they used in so doing. We also wished to see what means the children used to 'date' the different epochs, and what knowledge they had of actual dates. For this purpose a series of three pictures was shown. All were large (about 3′ × 4′), coloured, and historically accurate. As far as possible a definite historical event was avoided, and the scenes were those of the ordinary life of the period. The pictures were:

(1) Charles I with typical Cavaliers and Roundheads.
(2) Ancient Britons.
(3) A Tournament in the time of Richard I.

In connexion with each picture the children were asked to write the answers to the following questions:

I. Who do you think these people were?
II. When did they live? (Answer any way you like.)
III. What things in the picture tell you when they lived?
IV. Give the name of any man or woman who was alive when these people were alive.
V. Give the date at which these people lived. Never mind if you have given it before; give it again. If you do not know it at all, guess it, but then put 'G' by your answer.

Before the test began, a picture was shown as an example and the questions were answered orally by the class.

The results are best considered by taking together the answers to the same question for all three pictures, since by this means any differences due to freshness of acquaintance or to the skill of the teacher may cancel out. As the concentric method[1] of arrangement of the history syllabus is in force in this school, all the children should have learnt something of each of the periods on which they were questioned.

In answer to question I some children gave answers which were, as a matter of fact, not correct, but which could easily have been so. There was nothing in the picture to contradict such answers, though to a mind familiar with the pictures of English History, the right answer would assuredly have been suggested. For example, instead of Stuarts, French, Spanish, Roman Catholics, or "Raleigh's men" were given. Such answers were marked 'vague.'

Answers such as "Romans," "Britons," "Danes" or "Crusaders" to Q. 1 for the Charles picture, or "Romans" for the second, or "Danes" or "Napoleon's men" for the tournament picture were of course wrong.

The percentages of wrong (W), no answers (NA), vague (V) and right (R) answers to Q. 1 are given on the next page.

The drop in the 12-years' results is due to the backwardness of the children tested. They would otherwise have been in a higher form. The high scores in the answers to the questions on the Britons are probably caused by the attention that is given to this period in the Elementary Schools, and also by the greater facility with which more remote periods are differentiated. As in the 'completion' test, most absurdities were made in regard to the Stuarts, the period nearest and most like our own.

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[1] The 'concentric method' is that by which the history syllabus is so arranged that the same ground is covered in successive years, but in each year the subject is treated from a different point of view, or with wider or narrower scope.

Age	8				9				10				11				12			
No. of children	10				29				55				72				25			
	W	NA	V	R	W	NA	V	R	W	NA	V	R	W	NA	V	R	W	NA	V	R
Charles I	50	20	20	10	24	34	28	14	31	14	31	24	29	15	36	20	28	24	24	24
Britons	0	20	0	80	21	3	7	69	11	7	13	69	5	3	0	92	8	0	0	92
Richard I	40	10	0	50	21	38	10	31	44	14	13	29	59	6	11	33	60	8	0	32
Average %	30	16	6	46	22	25	15	38	28	11	19	40	38	8	15	48	32	10	8	47
% of W and NA	46½				47				40				36				44			
% of V and R	53½				53				59				64				55			

The increase in the number of 'vague' answers is probably due to the child's increase in scope of historical knowledge without at the same time being able clearly to differentiate between national types. It is interesting to notice that this 'vagueness' is highest at 10 years and that it decreases at 11 with an increase in correctness. This agrees well with the more positive knowledge which appeared in the other tests at this age[1].

In answering Q. II the children chose one of four different ways of marking the period:

(*a*) By a date.

(*b*) By an epoch. This was often vague. B.C. was counted as an epoch, as were also "Stuarts," "Stone age," "Middle ages," "Olden days," "Days of Chivalry."

(*c*) By giving the name of a person they associated with the period, *e.g.* "In the time of Charles I," or "When Julius Caesar was alive," or "In Robin Hood's time."

(*d*) By saying definitely, or indefinitely, how long ago, *e.g.* "2000 years ago," or "a long time ago."

The results for Q. II are shown on p. 326.

On these results two questions arise, (i) what means did the children use to mark the period? and (ii) with what degree of correctness did they use these means?

As to (i), the most noticeable fact is the great popularity of the Date or of a Person as a time indication. Of these two the Date is the more popular at the earlier ages, but it is so frequently wrong that it cannot

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V. Results of 'Julius Caesar' test and 'completion' test, pp. 319, 321, *ante*.

have much meaning. Between the ages of 9 and 10 a change occurs and the association between an epoch and a famous person of that epoch becomes the characteristic method of 'dating.' Of the other two methods "Long ago" is popular with the youngest children, but rapidly becomes rarer as they grow older. The Epoch is always rather unusual except at 10. This result may be due to accident, or it may indicate the point at which the change from the indiscriminate giving of dates is ceasing, and knowledge is hardly yet definite enough to allow them to name a Person in the period.

Age				8					9					10		
No. of children				10					29					55		
Percentage of children answering by:—																
		D	E	M	LA	NA	D	E	M	LA	NA	D	E	M	LA	NA
Picture I	2	0 *50*	10 *0*	10 *10*	20 *0*	0	3 *28*	7 *21*	7 *3*	21 *3*	10	0 *18*	7 *15*	24 *15*	14 *4*	9
„ II	2	10 *20*	20 *0*	0 *0*	20 *0*	30	18 *14*	14 *0*	10 *14*	7 *0*	24	27 *0*	33 *0*	4 *7*	7 *5*	18
„ III	2	20 *20*	10 *10*	0 *10*	0 *30*	0	13 *21*	4 *7*	14 *3*	14 *1*	25	4 *7*	11 *11*	29 *0*	4 *24*	11
Average %		10 *30*	13 *3*	3 *7*	13 *10*	10	11 *21*	8 *9*	10 *7*	14 *1*	30	10 *8*	17 *9*	19 *7*	8 *11*	13
Incorrect + correct added		40	16	10	23	10	32	17	17	15	30	18	26	26	19	13

Age				11					12		
No. of children				72					25		
Percentage of children answering by:—											
		D	E	M	LA	NA	D	E	M	LA	NA
Picture I	2	3 *15*	11 *14*	20 *11*	3 *11*	6	0 *12*	4 *4*	76 *28*	4 *0*	4
„ II	2	56 *0*	14 *0*	11 *3*	12 *3*	3	36 *0*	16 *4*	8 *4*	20 *4*	8
„ III	2	7 *7*	11 *4*	35 *21*	4 *1*	10	0 *12*	8 *4*	28 *20*	16 *4*	8
Average %		22 *7*	12 *6*	22 *12*	6 *5*	6	12 *8*	9 *4*	37 *17*	13 *3*	7
Incorrect + correct added		29	18	34	11	6	20	13	54	16	7

D = Date. E = Epoch. M = Man. LA = Long ago. NA = No answer.

Italic figures indicate incorrect answers.

With regard to the accuracy of the answers, there is a steady and rapid improvement in those answers which determine a Date by reference to a Person. Accuracy as regards the Date itself improves greatly between the ages of 10 and 11, but for the other ages is much the same. Accuracy as regards the Epoch is more irregular, being at its best at 10 years of age. These figures depend on the actual number who attempted to answer in these ways and do not show the comparative reliabilities of the different methods of answering. If we take the percentage of accuracy on the totals, the Date gives 47 per cent., a Person 65 per cent. and the Epoch 67 per cent. The wild use of the Date by the younger children recalls the absurd answers given in the 'questions' test to the questions about the time of day by the 4- and 5-year-olds. Evidently, exact designations of time pass through a stage in which such are known but little or no meaning is attached to them.

The answers to Q. III were interesting chiefly as showing a steady increase in ability to note definite marks of a period, and as showing that it was clothes more than anything else that attracted the children's notice.

The percentage of those who indicated definite things in their answer rose from 36 at age 8 to 72 at age 12, and the average percentage of those who noticed dress, either definitely or vaguely, was 80 as compared with 16 who noticed other things. 4 per cent. gave no answer.

In Q. IV there was a steady increase in ability to give a name in connexion with a period. At 9 less than half could give any name, wrong or right, by 11 only 22 per cent. failed to answer. At 9 years of age 18 per cent. of the answers were right, and 30 per cent. wrong; at 11, 49 per cent. were right and 29 per cent. wrong. The remaining percentage in either case gave no answer. It was noticeable that all the names given were those connected with battle or warfare, with the exception of King John (two children), Guenevere (one child), Wolsey (one child). Henry VIII and Charles I were also given. These have decided military connexions.

There did not seem to be any noticeable decrease in the number of guesses which children from 8 years old to 12 years old had to make in answering Q. V. The proportions who did not guess varied between 51 and 92 per cent. Dates which were not absurd were given in from 31–40 per cent. of cases. There was no clear improvement with age.

§ 7. *The 'temporal order of historical characters' test.*

The 'picture' test gave us some idea of children's power to distinguish periods and their method of indicating a point of time by the name of some prominent person or by a date. We next wished to see (i) what methods they would adopt if asked to arrange persons in sequence; (ii) in view of their general failure in the 'picture' test to manipulate dates, and the low degree of consistency achieved, whether an arrangement on a basis of dates would give the same result as an arrangement by some other means; and (iii) what would be done in the case of people whose dates were not known.

A list of five names was therefore given to the children, and they were asked first to arrange the people in the order in which they lived, then to assign them to their proper centuries, and then to give their reasons for the original arrangement. An attempt was made to do this as a group test, but it was found unsatisfactory, and the test was then given individually. The numbers are therefore smaller, but more information was obtained. The names usually given to the children were Julius Caesar, King Alfred, Robin Hood, Charles I, Tennyson, Admiral Beattie. They were varied according to the child's knowledge.

The reasons given by the children for their order of arrangement fell into three main types. In the case of a few of the elementary school children the people were arranged in the order in which they had entered the child's experience, *e.g.* "I put Robin Hood first because I heard of him in the Infant School, and I heard of King Alfred in the first form." This type of answer was never given at school Y. The most usual type was a rough dating by epochs, "Robin Hood lived in the time when there were few towns, he lived in the forests." In its most advanced form this type involved considerable reasoning. One boy from school Y said, "Tennyson comes late in the list. I have been reading his poems and they are not old-fashioned." A third group relied on some external means of arrangement. The simplest of these means is order in the history book. "King Arthur is before Charles because he comes earlier in our book." This can be carried over to Robin Hood who usually does not occur in the books. "He lived about the time of William II, so he is later than Alfred."

The most advanced form of this method is the true arrangement by dates. Only one child, a girl aged 9, was found to use this method fully, though another used it in some degree. There did not seem to be any form of arrangement characteristic of a special age. The differences seemed rather to be due to greater or less knowledge of the special people given; dates being more frequently used when the character happened to come from "our period" than when he did not. Very much the same thing occurred when a variety of the test was tried on three adults who were asked to arrange a fairly difficult list of names. It was found that a genuine historian at once assigned the names to their centuries and then put them in order. Another, who knew very little history, arranged them exclusively on the basis of epochs, though certain epochs had a vague date attached to them, *e.g.* Renaissance, 1400. A third, whose historical qualifications were intermediate, seemed to use a mixed system, relying on dates as far as she knew them, and then using epochs. This use of epochs in the earlier stages of knowledge is quite in accord with the greater ease of the points H, L and E in the 'Julius Caesar' test.

When the children were asked to arrange in order by centuries the names they had already arranged in order, it became clear how little importance the majority attached to dates. This was shown in various ways. There was very little correlation between success in the two tasks. Children who had the order quite or practically right, might get the dates all wrong. But more striking were the differences of order between the two arrangements. Of 14 children tested at school Y, three changed the order, although their previous arrangment was on the page before them. They did not appear to regard the second arrangement as an improvement on the other. At an elementary school, of 16 children tested, aged 8 and 9, eight gave a different order with the dates. Moreover, many of the dates were quite absurd and could only have been given if they had no meaning for the children. One boy, age 9, who must have known St John from scripture lessons, dated him at 1500. Another bracketed Joan of Arc and Lloyd George as both living in 300 A.D. Another gave St John his right date, but put Robert Bruce, Robin Hood and Charles I all earlier.

There was a general tendency to put unknown people early, which applied also to people who were known by name, but with whom the children connected no period. The other well-marked tendency was to group people of the same occupation together. For example, Chaucer and Milton usually occurred in close proximity, Drake and Admiral Beattie, and on two occasions Alexander the Great and Charles I were so grouped! This tendency is, of course, well known and widespread; it is seen in the seven sages of Greece and in other half-legendary characters whose dates are approximated.

§ 8. *The 'temporal memory' test.*

Given a reasonable understanding of the nature of time-words and relations, is it possible to show that time-marks possess less significance for children than other details, and disproportionately less for children than adults? Certainly adult forgetfulness for such details as the time of a meeting is very marked, and children seem to be even more unreliable. We used two tests for an investigation of this point. The first was the reproduction of a story that contained details of various kinds, our object being to see if one type of detail was forgotten more readily than another. The second was the composition of a letter asking a friend to come out for a walk, our aim here being to see if the children would remember to name both the time and place of meeting, or would forget one more often than the other. In the memory test various stories were used, the two main ones being *Archbishop John* and *Chenklo.* In all cases the story was read through once and the children were asked to reproduce it in writing after an interval of from 20 to 30 minutes. They were told in advance that this reproduction would be required. In marking, the stories were split up into what seemed separate ideas, and these were counted as correct even if the wording was not exactly right, or if they were displaced from their proper place in the story. The 'inaccuracy' of the memory of any point was taken as the percentage of times the point was wrongly remembered in proportion to the total number of times it was remembered at all. This percentage is later called the coefficient of inaccuracy.

A B C D E
Archbishop John | was a patron of art | in Denmark. | In the summer | of 1679 |
F G H I
3 | poor boys | came to ask him to help them to learn painting. | They arrived on
J K L M
Sunday | morning | at his palace | at Copenhagen, | and said they had walked | 152
N O P
miles to see him. | The servant | said the Archbishop would be busy all day. | They
Q R R_1 S
must come again | at 6 o'clock in the evening. | They amused themselves in the public
T
gardens | which were only 100 yards | away till the Archbishop could see them. | He
U V W
gave them each £27 | and made them stay in the city | during the month of June, | to
X Y Z α
see the great works of art | preserved in the churches there. | In after years | two of
β γ δ ε
them, | William | and Martin, | became painters famous | throughout Europe. |

1 2 3 4 5
Chenklo | was a brigand | who lived in a cave | on a rock | which towered 1120 ft. |
6 7 8
above the Caspian sea. | He was only 48 | but his hair was already as white as snow. |
9 10 11
17 devoted followers | were ever at his beck and call. | The cave was stored with gold
12 13
and jewels, | but he became ever sadder and more silent. | Twice a year, | once when
14 15 16
the first swallow returned, | once on the 8th of November, | he retired to the topmost
17 18 19
peak of the mountain | and spent two days | in lonely meditation. | None knew his
20 21 22
secret, | but in his last years | he retired to a monastery | and became a devoted servant
of the Church. |

It could not be expected that the time details, however unimportant they might be, should all be found together at the bottom of the list. With stories of this nature various circumstances may affect the position of the different points. *E.g.* a certain romantic interest seemed to attach to Chenklo's hair being white, and this probably affected the number of individuals who remembered his age. In another test the results of which

Points arranged in order of difficulty, beginning with the easiest.

Archbishop John.

I. 18 children, 12–14.

No.	Point	Percentage remembering it correctly	Coefficient of inaccuracy
G	Poor boys	78	0
A	Archbishop John	72	23
H	Ask him for help	72	0
O	The servant told them	72	0
δ	Became famous painters	61	0
P	Archbishop busy	61	0
F	3	56	33
U	Gave each £27	50	25
R	**At 6 o'clock in evening**	39	0
S	*Public gardens*	39	21
α	Two of them	39	0
C	*In Denmark*	33	0
L	*At Copenhagen*	33	0
M	They had walked	33	0
T	100 *yards*	33	33
V	*In that city*	33	0
γ	Martin	33	0
I	**Sunday**	28	0
Z	**In after years**	28	0
ε	*Throughout Europe*	22	0
N	152 *miles*	22	50
B	Patron of art	22	77
D	**In summer**	17	57
J	**Morning**	17	0
W	**Month of June**	17	0
K	*At his palace*	17	0
X	Great works of art	17	0
E	**1679**	11	71
Y	*In the churches*	11	0
β	William	11	71
Q	They must come again	6	0

II. 20 adults.

No.	Point	Percentage remembering it correctly	Coefficient of inaccuracy
δ	Famous painters	95	0
C	*In Denmark*	80	0
G	Poor boys	75	12
M	They had walked	70	0
α	Two of them	65	13
U	Gave each £27	65	7
B	Patron of art	65	19
H	Ask him for help	60	0
A	Archbishop John	60	37
O	Servant told them	55	26
F	3	50	50
γ	Martin	40	20
S	*Public gardens*	35	36
P	The Archbishop busy	35	38
L	*At Copenhagen*	35	12
E	**1679**	35	50
Q	They must come again	30	0
β	William	30	33
J	**Morning**	25	0
K	*At his palace*	25	0
X	Works of art	25	16
Z	**In after years**	25	16
R	**6 o'clock in the evening**	20	0
T	100 *yards*	20	20
W	**Month of June**	15	50
N	152 *miles*	15	80
Y	*In the churches*	10	50
ε	*Throughout Europe*	10	33
D	**In the summer**	10	0
I	**Sunday**	10	0
V	*In that city*	5	0

Points in order of difficulty.

Chenklo.

I. 73 children.

3	*Lived in a cave*	84	0
11	Cave full of gold	81	2
8	Hair as white as snow	73	0
16	*Went to peak of mountain*	66	6
7	**Only 48**	64	18
6	*Caspian sea*	60	15
12	Grew sadder	53	2
21	Retired to a monastery	49	18
2	He was a brigand	41	14
9	17 devoted followers	41	47
17	**For 2 days**	40	12
22	Servant of the church	33	27
13	**Twice a year**	31	18
19	None knew his secret	30	19
10	Followers at his call	29	5
4	*On a rock*	26	14
18	In lonely meditation	25	0
20	**In his last years**	18	53
14	**When the swallow returned**	14	50
1	Chenklo	11	87
15	**8 Nov.**	10	59
5	1120 *ft.*	8	88

II. 8 adults.

9	17 followers	88	0
2	Was a brigand	75	0
11	Cave full of gold	75	0
12	Grew sadder	75	0
13	**Twice a year**	75	0
16	*Peak of mountain*	75	0
21	Retired to a monastery	75	0
3	*Lived in a cave*	63	0
19	None knew his secret	63	0
4	*On a rock*	50	0
7	**He was 48**	50	43
8	Hair as white as snow	50	0
14	**When the swallow returned**	50	43
15	**8 Nov.**	50	43
22	Servant of church	50	20
1	Chenklo	32	50
5	1120 *ft.*	32	53
6	*Caspian sea*	32	0
10	Followers at beck and call	32	0
20	**In his last years**	32	40
17	**Spent 2 days**	25	0
18	In lonely meditation	12	0

Time in heavy type. Place in italics.

were closely similar (and which are therefore not given), an age not connected with any such detail came out bottom. Moreover, some points are more closely connected with the fabric of the story than others. *E.g.* the first five points in *Archbishop John* could hardly have been missed without destroying the story. But allowing for these causes of irregularity, it can be seen from the tables that the details of time and space do form a sediment, as it were, at the bottom of the lists, and that of these two, time holds a lower place than space. The percentage of people remembering correctly the different types of points is:

		Time	Space	Others
Ch.	Children	35·4	48·8	43·3
A. J.	Children	23·1	27	45·5
A. J.	Adults	20	26	54·6
Ch	Adults	47	50·4	55·2

It can be seen that space has always a superiority over time, and that both space and time are, except in one case, inferior to the other points taken together. In this one case it seems that the slight superiority of space is due to the fascination for children of 'life in a cave,' which heads the list.

The tables of the inaccuracy coefficients show that it is not to time or place as such that inaccuracy mainly attaches, but to all definite names and figures. With few exceptions, all the high coefficients (over 20 per cent.) belong to names or figures. In two of these cases, "patron of art," and "servant of the church," the mistakes were due to a failure to understand the phrases. The other three are marks of time: "in his last years," "in summer," and "the first swallow," and may perhaps be due to inattention to time. However, the numbers who remembered the existence of these points were small. In spite of the inaccuracy in recall, we do not find a sediment of names and figures at the bottom of the lists as we do with time and place. The mistakes in many cases suggest that it was the effort to recall the details *exactly* that proved difficult. The figures were represented by some vague phrase, "some poor boys" taking the place of "three," or "one day" taking the place of "in summer." It was not rare to get an "about" added to the round number, *e.g.* "about 1100 ft." as if the writer were rebuking the experimenter for being unnecessarily precise.

From this test then we can get some experimental evidence of the observed fact that time details are remembered with more difficulty than others, but the evidence does not show that this inability to remember is more marked in children of ages 12–14, the ages tested, than in adults. Such inability would probably have been shown more clearly if we had used younger children, but the difficulty of using the same tests on both groups would then have been too great. If, as was suggested above, time-knowledge begins to assume its adult form at about 11, this would account for the result.

The last test was intended to illustrate the same point, when the problem was not one of memory, but the formation of plans of future action.

§ 9. *The 'making an appointment' test.*

This test gave much more definite results. The instructions to the children were: "Write a letter to a friend arranging to go out for a walk with him." The letters were mostly quite short, and the greater number made a definite appointment to meet. In these cases it was possible to see how many gave both time and place of meeting, and how many forgot one or the other. Of 153 children of various ages tested, 128 gave a definite place of meeting, only 94 gave a definite time. Of the others, however, most gave an indefinite or inadequate time, *e.g.* to-morrow, or Tuesday afternoon. Very few gave absolutely no time indication at all. There is a slight improvement with age up to about 13. Owing to the conditions of the test the 14-year-olds were more stupid than the others. The numbers at 11 years of age are too small to be reliable.

Age	No. of children	% giving Time	% giving Place	Difference place-time
8	12	33	92	59
9	23	61	87	26
10	25	60	88	28
11	7	86	71	−15
12	32	72	94	22
13	32	59	66	7
14	22	59	87	28

Some children gave both time and place.

From these letters it seems possible to imagine how the plan presented itself to the children. In one case they thought of a vacant afternoon to fill, and the letters read, "Can you come for a walk on Saturday afternoon?" In others the object of the walk was more prominently in mind, "Can you come for a walk to Leigh woods?" Of the two the latter was the more likely to lead to the making of a definite appointment; in the former the writer seemed apt to give no further time indication, apparently feeling that what was so clear to him must be equally clear to his friend. We can conclude then, that in forming plans, time is less important to children than space, and that the tendency is very strong for a vague time to take the place of a definite one.

§ 10. *Summary of experimental results.*

From these experiments we can draw some general conclusions as to the growth of children's knowledge of conventional time.

(1) This growth is essentially a slow process, already beginning by the age at which our experiments started—four years—and arriving nearly at the adult level at about 13 or 14 years. The most important period in its development seems to be at about the age of 11, when there appears to be a rapid improvement in all types of time-knowledge.

(2) Children appear to learn first the meaning of the time-words in ordinary use. The order in which these are acquired appears to depend (*a*) on the periodicity of the cycle in which they occur, *i.e.* the parts of the day are learnt before the days of the week, and the weeks before the year; (*b*) on the personal interest involved, *e.g.* the date of a birthday is learnt early, also the occasion of such events as the weekly holiday.

(3) The day of the month always presents great difficulties, probably because of the highly artificial method of numbering and the lack of distinguishing activities for the different days.

(4) Marks of time are closely connected with activities or concrete experiences. *E.g.* the seasons are identified with characteristic natural phenomena, and such absurdities as B (day's march) in the 'Julius Caesar' test are comparatively easy of detection.

(5) Passing beyond the ordinary time-words to an understanding of chronology and the arrangement of historical epochs is a matter of some difficulty. Children find it difficult to arrange dates correctly, and apparently attach little importance to them when required to assign them to persons, or to arrange historical characters under their proper dates. When required to 'date' an epoch they prefer to give the name of some famous contemporary character.

(6) In dealing with historical epochs those which are most remote from our own time are most readily distinguished. This seems to be due to the fact that the earliest distinction is between the present and a past, which is mainly negatively characterized. In this past, subdivisions are at first ignored: they are only attended to after the age of 11 years. Children naturally find it easier to recognize the representation of an epoch, *e.g.* by a picture, than to give a description of one themselves.

(7) Indications of time appear to play a less important part in children's thought than do those of space.

§ 11. *General educational implications of the tests.*

The number of children tested is obviously too small to admit of drawing dogmatic conclusions; but a few hints as to history syllabuses, and methods can perhaps be gleaned, although it must be realised that these experiments tested the results of teaching, as well as natural interest and preference.

In the first place, it seems that up to 11 years of age in the elementary school, *i.e.* up to about standard IV or V, conventional names of time-periods, especially dates, have little meaning for children unless they are definitely explained and correlated with the child's own activities; *e.g.* when a week, or a year, or two years, or twenty years, is mentioned, it should be connected with the life of the child, mother, family, etc. Much might be done if the periods of time used in 'time' sums (usually taught in standard III) were made more meaningful. It is easy to show what a second or a minute means; and a little discussion on how long lessons last, how long school time lasts, how long since and before holidays, would all educate children in this way. Games and sports involving 'timing' are also helpful here. Definite and direct teaching on 'dates' is required whenever they are given in the lower standards. In view, however, of the apparent lack of interest in such conventional names, it seems that the picturesque and descriptive aspects of history are those which should receive most attention. Most of the time should be given to making the past as vivid as possible in concrete detail.

Since children at this stage seem to notice chiefly what people in bygone ages had *not*, rather than what they *had*, it seems that more emphasis should be laid on *positive* detail. The method should not be entirely that of noting the contrast between past and present, but should aim at building up a vivid and real conception of the past. The contrast is obvious to the child in proportion as both past and present become vividly known in detail.

Pictures, models, charts, dramatization, handicrafts, are all a means to this end. The chief fault which the writers have noticed in this connexion is the narrowness of scope in the details of previous epochs which are taught. This fault is most noticeable in history dating from the Christian era; it is not so apparent in the more primitive history.

Little ability to conceive of continuity and development is apparent up to 11 years of age. This should be left to the higher classes; but in the opinion of the experimenters time charts could be much more extensively used than they are at present. The concrete, pictorial representation of a conventional and abstract thing is thus given to the child[1]. In no school where these tests were carried out was a time chart used.

Since in many elementary school children there was a tendency to place epochs, events, and people in the order in which they had appeared in the child's own consciousness, and not in the real chronological order, the concentric method can perhaps be called in question; especially since the primitive and early historical periods appear to be most interesting to children. The use of a time chart might counterbalance the dangers of the concentric method.

The apparent development of a keen interest in people at the age of 10–11 years is also significant. In the experience of the writer it is then that 'heroes' either of fact or fiction evoke most admiration from the child. The second stage of history teaching, then, should centre in biography, as the first in intimate concrete details of typical historical periods.

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[1] A useful example is described by H. Madely, *School as a Training for Citizenship*, p. 38.

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THE SCHOOL IN THE SERVICE OF VOCATIONAL STUDY[1].

BY OTTO LIPMANN.

(*From the Institut für angewandte Psychologie, Berlin.*)

I. *The need for determining the nature and degree of endowment* (pp. 337, 338).
II. *Endowment includes capacities and interests. The 'material' and 'formal' aspects of endowment must be distinguished* (pp. 338–341).
III. *Quantitative and qualitative classifications of vocations* (pp. 341–343).
IV. *The value of such classifications for vocational guidance* (pp. 343, 344).
V. *The scope and methods of vocational diagnosis* (pp. 344–348).
VI. *The functions of the school in vocational diagnosis* (pp. 349–351).

I.

WE may distinguish a quantitative and a qualitative aspect of vocational study. The one seeks to establish the *degree* of individual endowments, the other the specific *nature* of these endowments. It has been assumed that the degree of the individual's endowments should constitute the only criterion as to whether the individual should in later life be called upon to adopt a higher, middle or lower vocation, individuals being accordingly drafted to such schools (in Germany *Gymnasium*, *Realschule* and *Volksschule* respectively) as would afford an adequate preparation for the corresponding vocations: while a knowledge of the *nature* of an individual's endowments has been supposed to be of importance for the choice of vocation within the general range of occupations suitable for the particular degree of endowment that the individual may possess.

The problem, however, is not so simple. We have largely ceased to regard schools as means of preparation for different classes of occupation; the idea of 'unitary schools' (*Einheitsschule*) is to a large extent now understood not only in the sense that schools should cease to represent any particular social class and that transference from one kind of school to another should be made easier, but also as signifying that the recognised demand for differentiation should be met not by a differentiation of pupils according to schools, but by a differentiation of pupils within each single school. A separation of pupils according to their degree of endowment would have as its consequences: first, that those classes which were denuded of the more gifted scholars would be deprived of the stimulating influence of leading personalities; secondly, that those vocations which are recruited from the lower schools, especially therefore the industrial and manufacturing occupations, would have to abandon all their leading personalities to the higher professions. Thirdly, there exists also the general question whether the concept of endowment can be completely expressed in quantitative terms, or whether we should not rather from the beginning distinguish certain qualitative types of endowment, such as practical and theoretical types, linguistic and mathematical types, etc. The idea of the 'unitary school' is at present therefore often understood in the sense that the pupils should be grouped according to the degree and nature of their endowments, and furthermore that the constitution of the groups should vary according to the subject of study; that one and the same pupil, for instance, may belong to a less advanced group in the case of linguistic studies than he does in the case of mathematics.

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[1] Translated by J. C. Flügel.

II.

Alike for these schools of the future and our schools of the present day there exists the necessity of forming a judgment as to the nature and degree of a pupil's endowments. Endowment includes both ability itself and willingness to use this ability, in other words both capacities and interests (in which statement we understand by interest the active and effective interests only). In the case of interest it is even more evident than in the case of abilities that nature and degree cannot be considered separately. Of a very few persons only can we say that they display a generally strong or generally feeble interest; in the great majority of cases preference is given to *one* field of interest, or else one or more fields of interest are neglected; *i.e.* the degree of interest differs according to the nature of the field in which it is exercised.

In considering the different kinds of endowment, with regard both to ability and interest, it is possible to distinguish the 'material' and 'formal' aspects of endowment. By the material aspects we understand the direction of endowment towards those fields of knowledge or activity, which as the result of our civilisation have developed into fixed 'professions' or 'occupations'; in this sense we may distinguish abilities and interests for mathematics, languages, music, sculpture, electrical engineering, architecture, carpentry, shoemaking, etc. It can *not* be the function of the school to establish the existence of these material endowments, *i.e.* to enter on vocational study in the narrower sense of the term, and this for several reasons.

A. The school has no opportunity to observe the behaviour of pupils with regard to these material fields of endowment. This applies both to the various handicrafts and commercial occupations and also to the arts and sciences: the work in school subjects often differs, not only in fact but even in principle, from work in the corresponding branches of knowledge and science. The science of higher mathematics has but few points of contact with the elementary mathematics which is taught in school. The way in which natural science and linguistic science are treated in school gives the pupil no conception of the scientific treatment of these subjects: though it is of course true that the 'work school' (*Arbeitsschule*) may help to bridge over this gap.

B. It would in many cases be difficult if not impossible to determine unequivocally the degree of endowment for any one of these material fields, since endowment is composed of both abilities and interests, and these exhibit no necessary correspondence one to another so far as these material fields are concerned. I need only call to mind Goethe's interest in pictorial art—an interest that was quite disproportionate to his productive abilities in this field. Many similar instances could be given; as regards myself for instance, I may say that I have no ability, even of a purely receptive kind, at all corresponding to my interest in higher mathematics, while on the other hand a certain linguistic ability that I possess is unaccompanied by any corresponding interest in the acquirement of foreign languages.

C. It is only within very narrow limits possible to predict from the presence of such endowments as can be diagnosed at an early age, the preponderance of similar endowments in later life. This is true more especially of the direction of *interests*, which, as is well known, are in youth liable to frequent change as regards their material aspects and sometimes undergo complete reversal. But even the material *abilities*, *i.e.* just those factors which in the opinion of many constitute vocational aptitude, are exposed to change as a result both of development and of the actual exercise of a vocation—changes the extent of which is at present so little known that conclusions made in early youth as to supposed vocational aptitude can possess only a very limited value.

We do not wish to deny that for the successful practice of certain vocations a certain measure of such material abilities, *e.g.* visual discrimination, dexterity, attention, memory, etc., is necessary: but in addition to the already mentioned difficulties of diagnosing these abilities in youth, there has also to be taken into account, so far as the school is concerned in this diagnosis (that is in so far as we are dealing with the choice of a suitable career for a given individual, and not with the selection of suitable individuals for a given career), the further fact that the relevant psychological concepts are by no means unequivocal. The question whether there is *one* memory or whether there are different memories for different fields, each of which may undergo a different degree of development, is one that has long been under discussion in psychology; and it is at any rate questionable whether we ought not similarly to distinguish between different kinds of visual discrimination, attention, or even dexterity[1].

The position is more hopeful as regards psycho-physics, *i.e.* in matters concerned with purely sensory functions, such as visual, auditory and tactual acuity; but psychology is only partially concerned with the measurement of these functions, which—as in the case of general health, heart, lungs, etc.—fall more within the province of the doctor, that is, so far as the school is concerned, of the school doctor. The doctor may however be very considerably assisted in his work by the teacher, who may draw attention to any defects he has observed in his pupils.

For the reasons which we have already considered it is impossible, in dealing with the choice of vocation, to attach any considerable importance to the supposed interest of the young person for this or that particular vocation; since this interest is in the great majority of cases determined purely by the material aspects of the vocation, *i.e.* it is associated with the *material* or *products* connected with the vocation, such as shoes, cakes, houses, electrical apparatus, etc., and not with the actual *nature* of the work with which young people are for the most part quite unfamiliar.

We may therefore conclude that any determination of vocational ability undertaken with a view to the choice of occupation on the part of those who are still young must aim at a diagnosis of the 'formal' endowments only. This is a task which should be performed by the school, if it undertakes to give advice as regards the choice of occupation; it is a task too, which it should be possible for the school to carry through to a very considerable extent.

We must now endeavour to make clear what is to be understood by

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[1] In view of the generally assumed close relationship between the requirements of *particular* vocations and certain *definite* forms of these psychical characteristics, we may include these abilities among the *material* endowments.

'formal' endowments. I am not in a position at present to make more than a rough preliminary sketch of this field, since the investigation of endowments, so far as it is connected with the study of vocational ability, has hardly yet approached the subject of formal endowments. What I have to say on the matter is therefore far more in the nature of a programme for research than of a report of the results of research.

III.

In dealing with formal endowments, we may retain the customary division into higher, middle and lower vocations, though in doing so we must use these concepts in a definite and special sense. The height of any vocation in our scale is determined by the extent to which the worker in that vocation acts 'freely' in virtue of his intelligence or imagination, working little with fixed rules and within fixed limits.

It is clear that some vocations admit of such 'free' action to a greater extent than others—the freedom being as a rule greater in learned and artistic than in industrial occupations—and that some men are more capable and more inclined than others to work on individual lines. A man is the more adapted to a vocation which permits or demands work of this free and individual kind, the greater his intelligence and imagination (both of which are 'formal' characteristics) and the greater his interest in using the abilities for the free manipulation of given material. Men of this kind should avoid vocations in which the nature of the work is prescribed in all its details, though it is far from being necessary that they should all adopt a vocation of academic or artistic nature; since commercial and manufacturing vocations are also capable of being developed into higher professions. Such men, on the other hand, as are lacking in the characteristics we have just described should of course avoid the learned and more especially, the artistic vocations.

Alongside of this more or less quantitative classification of vocations, it is possible to make qualitative classifications, and this from various points of view. Without going into detail, a few such classifications may be enumerated here. All the categories to be mentioned refer to formal endowments, both as regards abilities and interests. Among the oldest of such classifications—one that is usually confined to the higher professions but which holds good also for many vocations of intermediate standing—is that which divides activities into those of a 'productive,' 'reproductive' and 'receptive' nature. Another classification, related to the above, regards vocational activities as 'gnostic' (aiming at knowledge), 'technical' (aiming at the achievement of results) or 'symbolic' (representing the inner life of man, artistic). We may further distinguish 'theoretical' and 'practical' vocations, according as they demand—in general or with special reference to the higher professions—more theoretical or practical intelligence[1].

This last-mentioned classification of vocational activities and the corresponding endowments constitutes in some respects a transition to the material point of view. This is true perhaps to an even greater extent of the further classification which treats occupations under different heads according as they deal with persons, things or thoughts.

The value of the classifications we have so far mentioned lies chiefly, though not exclusively, in their application to the higher professions. The following classifications are perhaps of greater importance in dealing with the occupations of intermediate rank. These vocations are classified according as they involve more manual work or more mental work; exact reproduction of given models or work of a 'freer' kind; rough or fine work; work that is executed under detailed instruction or work that is executed independently; long continuous processes or frequent short processes; frequent repetition of the same activity or frequent change of activity; concentration of attention on a narrow field or distribution of attention over many details; work in which each individual acts alone or work in which he acts in intimate cooperation with others; solitude or companionship with one or several fellow-workers; work out of doors or work indoors (room, factory, machine shop, etc.). It is difficult to bring all these points of view within a single system. An attempt at such a system is that which has been made by the *Institut d'Orientació Professional* in Barcelona. In this the vocations are classified according to the types of intelligence, temperament and character that they require. Work that is principally 'mental' in nature requires abstract intelligence, 'psycho-physical' work requires verbal intelligence, while work that is principally 'muscular' requires technical intelligence. Temperaments which easily admit of automatization are fitted for work of a *fixed* and *definite* kind, in which the various processes are constantly repeated; while temperaments of the opposite type are suited to work of a more *variable* kind. Work is further of a 'perceptual,' 'perceptuo-motor' or 'motor' character, and this last classification is made to correspond to similar types of character.

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[1] Theoretical and practical intelligence differ principally, according to our view, in the two following respects.—(i) In a task which involves theoretical intelligence, the understanding of the task itself makes demands upon intelligence, whereas practical intelligence is concerned only with the execution of the task. (ii) In tasks demanding only practical intelligence both the starting point and the goal are usually given in terms of *perception*, whereas in tasks demanding theoretical intelligence, these are mostly given in terms of *thought*.

These various formal characteristics of a profession do not of course constitute a complete 'vocational picture,' but they do include the majority of the essential points involved in the choice of vocation. Vocational study must therefore be based on the investigation of the endowments corresponding to these various vocational characteristics.

IV.

Before we enter upon the problem as to what means exist in the school for the determination of these types of endowment, there remains a question of fundamental importance to be settled. It is clear that not everyone can be easily assigned to one or other of the above-mentioned types, and this is true also of some of the young persons in need of vocational advice. What significance then have the various characteristics that we may have assigned to the various occupations, for the determination of mental traits with a view to vocational advice? In my opinion the importance of these characteristics lies principally in this; that with their help we are enabled to recognise, on the one hand those who are specially suited (or so to speak predestined) for particular occupations, on the other hand those who are quite unsuited for particular occupations. Any determination of traits based upon these characteristics is not quantitative in the sense that the presence of a characteristic in a particular degree implies a precisely corresponding degree of suitability for a profession. These characteristics permit rather only of a differentiation of individuals into those who are specially suitable, indifferent and definitely unsuitable; and it must be added that the group of indifferent individuals will be by far the largest of the three. Not all occupations demand specific qualities, and not all men possess specific endowments. Vocational Psychology must needs concentrate itself on the relatively few remaining occupations and individuals, in which specific qualities (positive or negative) are needed or are present.

Quantitative determination of vocational aptitudes is of course within the bounds of possibility, but this will be a matter of definite selection rather than of guidance and advice, and therefore falls no longer within the province of the school. Quantitative degrees of vocational aptitude, especially those within the group of indifferent individuals, would relate in the first place to certain of the above-mentioned material abilities, certain forms of visual discrimination, dexterity, power of optical combination, etc., in the second place to general intelligence, theoretical and practical. I am indeed inclined to attribute greater importance to intelligence than to the material and more elementary specific abilities, that is at least where the above-mentioned conditions with regard to formal qualities are fulfilled; since an intelligent person will often be in a position to compensate for the lack of such specific abilities as may be insufficiently developed, by his use of other abilities of a different kind. Such vicarious function however is only possible where the vocation is of such a nature as to admit of a certain degree of modification to suit individual needs and capacities, that is, is capable of being developed into something resembling the 'higher' professions. It follows from this that the degree in which the material specific abilities are present is of greater importance for vocational selection, the lower and more mechanical is the class of vocation concerned; while the degree of intelligence, on the other hand, becomes of ever greater significance with increasing 'freedom' of the occupation. From this point of view, therefore, it is possible to establish a scale of vocations, which would indicate what degree of intelligence is required or is utilisable at any particular point of the scale; such vocational scales have already been drawn up in America in connexion with the army intelligence tests.

It is the aim of vocational diagnosis to discover the nature of an individual's endowments before his professional career has actually begun. In so far as the relative endowments are of a formal nature, there are many opportunities of different kinds for their detection; but in so far as they are of a material nature, they can be recognised only with the help of a more or less exact reproduction or imitation of the relevant vocational activity—either as a whole or in its constituent parts.

V.

The methods of vocational diagnosis may be classified according to various points of view:

I. The behaviour of the subject may be:
  (1) spontaneous,
  (2) reactive; in which case the procedure may be:
    (*a*) unsystematic,
    (*b*) systematic,
      (α) individual examinations,
      (β) collective examinations.

II. The behaviour may be studied:
  (1) by introspection,
  (2) by external observation, on the part of:
    (*a*) the teacher,
    (*b*) the parents,
  (3) by quantitative measurements of ability.
III. The results of the study may be assessed:
  (1) by self-judgment,
  (2) by external judgment, on the part of:
    (*a*) the parents,
    (*b*) the teacher,
    (*c*) the vocational advisor.

We will here examine more carefully a few of these possibilities.

Great importance, especially for the diagnosis of formal endowments (abilities and interests), attaches to spontaneous behaviour. Has the young person any spontaneous interests; if so of what kind? To what kind of activities do they lead him? What are his achievements in the fields of his spontaneous interests? We have here to rely, at any rate to some extent, upon the child's own statements, especially if we extend our questions, as we usefully may, so as to discover what interests he would *like* to call into play, if he had the necessary opportunity. Apart from this last additional field of inquiry however, much useful material may be gained from observations on the part of the parents; though the *evaluation* of such material, both that which relates to the interests and (more especially) that which relates to spontaneous activities, must be left to some more objective source of judgment. Parents have often no objective standard of comparison, and they are for obvious reasons frequently inclined to overestimate both the strength of the interests and the quality of the performance. Even less reliance can be placed upon the introspections of the young people themselves. We must not ask a child what interests him, but rather what he does, or what he would like to do. It must not be forgotten, moreover, that every judgment represents the conclusion of a process of induction based upon individual observations, and that the question as to whether the available data justify the conclusions is one of great difficulty, one which must certainly not be left to the discretion of the children themselves, nor (in most cases) to that of the parents, and which can be entrusted even to the teacher only under reservation.

Of principal importance for the school itself is the study of the reactive behaviour of the pupil. Such study must rely chiefly on casual opportunities for observation in the course of school life, not only in the class room but also during free intervals, school excursions, etc. We are here concerned not so much with the interests shown in the various subjects of instruction as with the above-described formal types of endowment. So far as material endowments are concerned, new opportunities for observation should, as we have already indicated, result from such modifications in the existing methods of instruction as are contemplated in the 'work schools.' As we have also mentioned above, the material interests directed towards definite occupations are often accompanied by lack of knowledge concerning the real nature of the activities involved in these occupations. Here the school can also be of assistance by giving pupils the opportunity of acquiring this knowledge. This may be done in three ways:

A. By actual instruction; the nature of vocational activities being made a subject of school study. For the reasons already given, the description of physical products and processes is for this purpose less important than that of the actual nature of the work involved in the vocation.

B. This is the case also where the instruction is accompanied by illustrative material, such as can be provided, for example, by the cinematograph. Such 'vocational films' however must in the first place be more than mere 'propaganda films': they must on the contrary give a true picture of the difficulties of the work. In the second place they must illustrate not so much the processes performed by the *machines* as the duties of the *workmen*. The so-called vocational films which have been produced up to the present are seriously deficient in these two respects. The cinematograph could nevertheless be so employed as to be of very great assistance in this field.

C. Better than abstract teaching, better even than teaching with the help of illustration, is instruction by means of actual practice. The school could be of immense service to vocational diagnosis, if it possessed workshops of its own, in which pupils could learn from their own experience the nature of the principal industrial and manufacturing activities. The actual performances of the pupils in different fields could then be objectively determined, while at the same time—and this I consider even more important—the pupils themselves would have an opportunity of judging which occupations they found pleasant and which disagreeable.

There is no sharp division but rather a continuous transition between casual and unsystematic opportunities of observation on the one hand

and systematic experimentation on the other. Every verbal testing of a pupil in class may of course be regarded as an individual experiment *in nuce*, while all written work or home work represents a sort of mass experiment. The difference between experiments of this kind and experiments in the strict sense—psychological tests in particular—lies, first, in the relatively inexact form, the less strict conditions of testing, the less objective estimation of results that characterize the school tests; secondly, in the nature of what is tested. School tests are essentially tests of knowledge, while psychological tests aim at measuring ability. While this distinction between school tests and the tests of experimental psychology is true in fact, it does not correspond, or at most corresponds only in part, to any *necessary* difference in the circumstances of the two kinds of experiment.

We must of course deprecate any attempt to turn the school into a psychological laboratory; the school must never lose sight of its own purposes—instruction and education, and everything the school does must, directly or indirectly, have reference to these two ends. While it is at liberty to undertake the task of diagnosing the endowments of its pupils, it is only justified in doing this in so far as this task has a bearing on instruction and education. We should be carefully upon our guard lest, neglectful of this principle, the school should overstep these limits; as would be the case if it were to place itself *directly* in the service of vocational diagnosis.

Nevertheless, a great portion of such diagnostic material as the school may collect for its own purposes, can be made of use for purposes of vocational guidance; and it is to the advantage both of the school itself and of the wider interests extending beyond the limits of the school, that this diagnostic material should be obtained by the exact methods of psychological experimentation and should not be confined to the mere examination of the pupil's knowledge. Attempts in this direction have indeed already been made. Lessons in drawing can, for instance, quite legitimately be made to include exact measurements in visual accuracy, especially since such diagnostic 'experiments' can easily be made to take the form of 'exercises.' Similarly in geometry, it is possible to use problems of the kind that are known in psychology as tests of the power of visual combination. In linguistic examinations, use can be made of the definition test, the three-word test, the word-completion test, the criticism test, etc. Even certain association tests can be employed here, such as those which consist in supplying a superordinate, coordinate or subordinate concept, the finding of parts for given wholes or wholes for parts, suitable adjectives or verbs for given substantives, etc. In general it is advisable to use also for purposes of examination problems of a kind that have not yet been used in the course of the actual instruction, but which can be solved by methods analogous to those that have already been practised: in this way the examination ceases to be a mere test of knowledge and becomes a test of ability. As to fields where this is possible, I need only refer to arithmetic, where analogy problems of this kind can easily be arranged in any number, and to grammar (both of the mother tongue and of foreign tongues), where the rules of declension and conjugation can be applied to new words in the case of which there has been no previous practice.

It is perhaps unnecessary to enter into the question of the respective merits of individual and mass examination, since both kinds are always employed in schools. The mass examination in the shape of written class work has the advantage that the same questions can be given to all the pupils, so that an exact comparison of the results becomes possible. All mass experiments suffer however from the disadvantage that it is impossible to study the different methods of work adopted by the individual pupils. Such study is moreover of great importance, since two results of equal merit may have been achieved in very different ways, and since the comparison of different methods of working may lead to practical conclusions of the greatest significance. The aim of our tests does not lie merely in the obtaining of numerical results and in the indication of a pupil's merit by means of a figure. Much more important even here are the qualitative observations that may be made during the tests. It is desirable that all instructions for the carrying out of tests should contain a list of the points in the behaviour of the subject which the experimenter should specially observe while the former is engaged on the test.

Although the possibility of making such observations confers an immense advantage on the individual test, we must on the other hand not overlook the fact that in these tests it is often impossible to give the same experimental material to all the pupils who are to be tested. This drawback can and will be gradually diminished, in so far as we are able to have at our disposition a large quantity of standardized material for use in our tests. The results obtained by any individual subject will then be compared, not with those of other subjects tested on the same occasion, but with a group of results obtained at other times and other places.

VI.

All this applies not only to tests within the school, but quite generally to all examination for purposes of vocational diagnosis. School tests have the disadvantage that they are not carried out by psychologists but by teachers. Even outside the school, however, vocational tests are, unfortunately, not always conducted by psychologists; and the teacher has at any rate the advantage over the engineer that he is accustomed to young people. Such psychological training also, as is necessary for the conduct of psychological experiments, is, in virtue of his professional education, more easily acquired by the teacher than by the engineer. Further, test experiments of the kind we have been considering must be made to fit into the general plan of the school, and must be carried out so often that, in the first place, the errors of the individual measurements will cancel one another, and, in the second place, the teachers will in the course of time acquire a mastery of the *technique* of experimentation. Finally, as already said, we attribute the chief importance, not to the numerical results of the tests, but to the observations that accompany them. The evaluation of these results and observations must of course not be left to the teacher but entrusted to the expert vocational advisor.

The great advantage of school tests as compared with those performed outside the school lies in the fact that the school need not be content with a single experiment, but can repeat the tests again and again and can observe the behaviour of pupils over very long periods.

As the result of work done in the *Institut für angewandte Psychologie*, I am inclined to be very sceptical as regards the diagnostic and prognostic value of single experiments. In so far, therefore, as single experiments may be absolutely necessary for purposes of selection, we must insist that the experimental results shall be supplemented by observations made in school. The *Hamburg Psychological Laboratory* has obtained excellent results from such a combination of experimental measurements and school observations.

In order to increase the value of these latter, the observations should not be made *ad hoc* just before the examination, but wherever possible should extend over the whole school period and should be undertaken and recorded not by one teacher only but by all those who have had to deal with the pupil at his various stages of development. As shown by our earlier considerations, however, the observations need only be made with reference to such (formal) qualities as are clearly related to certain types of endowment, and therefore also to such individuals only as show themselves clearly to belong to one or other particular type of endowment. The demands made upon the teacher would not therefore seem to be excessive. It will be well to provide the teacher with a definite set of instructions as to how the observations should be carried out (several provisional sets of instructions of this kind have already been drawn up) and to make it part of his official duty to carry out and record his observations in accordance with these instructions. The extent to which the observations should relate to the pupils' spontaneous behaviour respectively may well be left to the discretion of the teacher. In every case, however, he should confine himself as far as possible to the collection of facts, leaving to the vocational advisor the task of drawing conclusions from these facts.

Under no circumstances should the school itself undertake vocational advice. Its only function in this respect is to provide vocational advisors with a part of the material upon which their advice must be based—a part only, for such facts concerning individuals as the school can furnish do not constitute the whole of the data required by the advisors, who must review these facts in the light of the knowledge of the psychological requirements of the various professions and training centres, and who must also bear in mind the economic factors concerned in the choice of a profession, the future opportunities presented by various careers, the economic circumstances of the moment, etc. It is the special professional duty of the vocational advisor to be well-informed on all these matters, and it would mean that a double duty would have to be undertaken by the teacher, were the school itself to attempt vocational advice. Such a course seems to me impossible, especially for the reason that vocational advice is necessarily concerned not only with professional activities but also with the process of training. Many of the qualities which can and should be made subjects of observation in school are of importance not only for the choice of vocation but also, and in an even higher degree, for the selection of a suitable place of training. Thus a pupil who is adapted to solitary and individual work had best be apprenticed to a private craftsman, while another who easily adapts himself to a group of fellow workers and who is generally of a sociable character will do better in a factory. Further, experience has shown that vocational advice should be accompanied by the recommendation of a suitable training centre, since the former when unaccompanied by the latter is left hanging in the air without the means of being put into immediate execution and is often totally neglected; in the absence of such recommendation, the pupil will frequently adopt any means of training that may happen to present itself, even if he is thereby adopting the most unsuitable career that could be imagined; or else he enters upon some unskilled occupation that requires no training.

Herein also, as I should like to emphasize by way of conclusion, the school may perform a very valuable service in the cause of vocational guidance, in that it may impress upon its pupils before their departure the general importance of vocational advice, may urge them not to decide upon the choice of a career without consulting an expert who will be able to give advice based upon a knowledge of the pupils' characteristics and of the actual economic situation, and may warn them not to leave this momentous decision (a decision that may involve the success or failure of their whole life) to chance circumstances, to a passing mood, to the suggestions of an unqualified advisor or to the influence of mere custom or convention.

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THE DEFINITION OF THE WORD AND THE SENTENCE[1].

By ALAN H. GARDINER.

It ought not to have been very difficult, one would have thought, to analyse the difference between a word and a sentence. Everyone, whether philologist or layman, knows that these two have, so to say, a quite distinct 'feel' about them; 'yonder house,' or simply 'house,' yields no satisfaction, while 'yonder house is Tom's' does so, and it seems reasonable to suppose that the presence or absence of satisfaction is a vital feature in the distinction which we make in using the terms 'word' and 'sentence.' Yet when such great authorities as Hermann Paul and Wilhelm Wundt are consulted, we find this point wholly ignored. Wundt defines the sentence as "the splitting up into its parts of a whole that is present in consciousness[2]," and Paul defines it, in much more cumbersome fashion, as "the linguistic expression, or the symbol, for the fact that several presentations or groups of presentations have become combined in the mind of the speaker, and the means for producing a like combination of the like presentations in the mind of the listener[3]." A simple example will, I hope, finally demonstrate the insufficiency of such pedantic definitions. If I say to a friend 'come for a walk,' it is indeed clear, with Wundt, that the notion which I have in my mind is communicated to my friend piecemeal, but that friend would be very much hurt if he thought that my sole aim in thus speaking to him were to chop up for his benefit a transient phenomenon of my consciousness. Obviously something is left out in Wundt's definition, and what is left out is precisely any explanation why a sentence, as opposed to a word or a mere combination of words, always seems in a certain measure 'satisfactory'—satisfactory, that is to say, inasmuch as it is self-sufficient and complete psychologically and socially, even when, as occurs often enough, it may also provoke resentment or dissatisfaction of one kind or another. The same objection holds good of Paul's definition: when I say 'come for a walk,' there is evidently something more intended than the mere communication from speaker to listener of a notion that has occurred to the former; somehow or other the dynamics of the situation is neglected in this definition, just as in that given by Wundt. The insufficiency of Paul's analysis is drastically revealed when such a question as 'is this Jones' house?' is compared with the statement 'this is Jones' house.' Here the same presentations or groups of presentations are combined in the speaker's mind and also conveyed to the listener, and yet the two sentences have an utterly different import. Could there be clearer evidence that the essence of the 'sentence' must be sought in some other direction? None the less, I have looked in vain for any more convincing analysis. The definitions due to English scholars are simpler, without answering objections of the type already brought; so Stout, "The sentence...is the expression of a unit of thought"; and Sweet, "The sentence is a word, or group of words, capable of expressing a complete thought or meaning[1]." But why do we go about troubling our neighbours with our units of thought? Why do we inflict our 'complete thoughts' or 'meanings' upon them? Ought we not to be content with mere thinking, instead of launching our thoughts upon the air? Again, the often expressed view that a sentence alone can have meaning or convey a thought seems to attribute to the terms 'thought' and 'meaning' too restricted and technical a signification. To me, at least, it appears that if I say 'yonder house,' pointing at the same time, I do express a meaning and, in so far, convey a thought, though that meaning or that thought certainly does not constitute a sentence.

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[1] A paper read before the Philological Society, Dec. 2, 1921.

[2] "Die Zerlegung eines im Bewusstsein vorhandenen Ganzen in seine Teile." W. Wundt, *Völkerpsychologie: Die Sprache*, 2nd ed., II, 241.

[3] "Der Satz ist der sprachliche Ausdruck, das Symbol dafür, dass sich die Verbindung mehrerer Vorstellungen oder Vorstellungsgruppen in der Seele des Sprechenden vollzogen hat, und das Mittel dazu, die nämliche Verbindung der nämlichen Vorstellungen in der Seele des Hörenden zu erzeugen." H. Paul, *Prinzipien der Sprachgeschichte*, 4th ed., IV, 121.

In a recent volume of the *Indogermanisches Jahrbuch* (VI, 1918), the various definitions of the sentence have been collected and critically examined by a scholar whose name I do not remember. Unfortunately, I have been unable to obtain this article, so I pass at once to my own opinions, which are the result of a strenuous effort to rid the problem of all abstractions, to look at Language in its setting of real life, and to regard sentences as the social facts, familiar from daily intercourse, which they obviously are. There are a thousand sentences spoken for every one that is written, and surely the right method of approach is to investigate speech, not as we find it in books or grammars, but as perhaps the most vital of all social phenomena, as the indispensable instrument of our social existence, where we use sentences at every moment to give or to ask information, to express wishes or give orders, or in some way to exert influence upon the minds of our fellows. Most writers on Language have, of course, been more or less alive to this standpoint, but Marty alone, so far as my reading goes, is entirely impregnated with it. His statement of the purpose of Language[1] agrees closely with my own definition, which runs: *Language is the name given to any system of articulate symbols having reference to the facts of experience, whereby speakers seek to influence the minds of listeners in given directions.*

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[1] These and other definitions are collected conveniently by A. Noreen, *Vårt Språk*, V, 51–61. My friend Dr Grapow points out that Paul Kretschmer has understood the sentence in very much the same manner as myself but has omitted the essential reference to the listener in his definition; see Gercke-Norden, *Einleitung*, I, 516–7.

This definition adds at least one new dimension to Language as usually viewed, namely the relation of speaker to listener. I do not mean to say, of course, that the relation of speaker to listener (with the converse relation) has been completely neglected by philologists; it is constantly cropping up in Paul, in von der Gabelentz, in Jespersen and in many other writers. But I do maintain that it is only very inconsistently kept in view. Still more is the neglect of this relation evident in the treatment accorded to Language in other sciences. Formal Logic is, as ever, the worst offender[2]. Formal Logic lives, if I may be pardoned the expression, in a holy terror of becoming verbal; but as she pretends to abstract from words themselves, while never ceasing to employ them in framing her propositions and syllogisms, what hope is there that she should pay any attention to either speaker or listener? As regards psychology, I would not venture to dogmatize in so disrespectful a manner; but a few recent excursions into psychological literature suggest that here too there is more than an inclination to identify speech completely with thought, and so to ignore, if not the actual audible and articulate character of speech, at any rate the attitude to a listener which speech essentially implies[3].

The four elements always present in normal speech, *i.e.* in speech as used in any social *milieu*, are (i) the speaker, (ii) the listener, (iii) the things spoken of, and (iv) the actual verbal symbols or words. With these elements in mind we shall find it easy to give, not indeed quantitative, but at any rate qualitative definitions of both the word and the sentence. Language itself seems to retain a consciousness of the four distinct elements composing it, for when the verbal symbols have as their object of reference the speaker or the listener, these are not spoken of as 'they' or 'it' like any other thing or persons, but have designations of their own, namely 'I' and 'thou.' The fourth element, the verbal symbols, do not likewise claim a personal pronoun of their own, since their essential rôle is to efface themselves in favour of the meanings which they carry.

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[1] A. Marty, *Untersuchungen zur Grundlegung der allgemeinen Grammatik und Sprachphilosophie*, I, 22.

[2] See Schiller, *Formal Logic*, 1912, for a trenchant condemnation of this discipline.

[3] In the symposium entitled *Is Thinking merely the action of Language Mechanisms?* (this *Journal*, XI, 54 foll.) none of the writers lays stress on the fact that one of the stimuli practically always present when the language-habit comes into play is the proximity of a listener. Would Professor Watson's infant run all over the house crying 'box' (p. 86) unless there were either actually or potentially present some person accustomed to act upon the suggestion thus given?

The principal types of sentences are statements, affirmative and negative, questions, imperatives with other hortatory forms of speech, and exclamations. The exactness or exhaustiveness of this classification does not matter for my purpose. Clearly the difference between these various kinds of sentence resides in the difference in each case of the speaker's attitude towards the listener: at one moment he supplies the listener with information, either by stating a thing or else by contradicting an erroneous impression; at another moment the speaker seeks information or addresses a command, or else makes an ejaculatory appeal for sympathy. These are, of course, by no means all the attitudes to a listener which speech can express; but for jests, taunts, threats, sarcasms and so forth Language has created no special forms. Now, since the various species of sentence, as above enumerated, have been universally recognized, it is little short of marvellous that the character common to them all has been universally overlooked. Clearly that common character must constitute the essence of the 'sentence,' and this essence consists in the presence of some volitional attitude towards a listener on the part of the speaker. Of course a sentence always does more than convey a volitional attitude of speaker towards listener; it always contains a reference to certain things or ideas spoken about. But to contain a reference to things or ideas is the characteristic of 'words'; every sentence consists of one or more words, though not every word is a sentence. My definitions of 'word' and 'sentence' therefore run:

A Word is an articulate sound-symbol in its aspect of denoting something which is spoken about.

A Sentence is an articulate sound-symbol in its aspect of embodying some volitional attitude of the speaker towards the listener.

Let us now put these definitions to the test. I begin with the stock-example of the word 'fire.' If I utter this word quite irrelevantly, at least it has what may be termed dictionary meaning. It may, however, mean more than this; it may actually refer to a specific fire, if such is in my mind when I utter the word. But as yet the word gives no satisfaction, it is not a sentence; it states nothing, asks no question, gives no command, in fact has nothing about it which can suggest why the speaker thrusts it upon the attention of the listener. The listener, if there is one, may remark "what about fire?" This question, when analysed to the bottom, means "What do you mean *me* to understand about fire? How am *I* intended to understand the word fire?" Otherwise expressed, the listener demands some volitional attitude on the part of the speaker which will somehow involve and concern the listener himself. Now the word 'fire' may be spoken in such a tone, and in such surroundings, as leave no doubt as to the speaker's meaning. If he utters the word while rushing from a house with a scared face, it is quite obvious that he desires to communicate the fact that the house is on fire; or if he is an officer addressing the word to his men, arms having already been presented, then it is obvious that his intention is to give a command. In both cases 'fire' is at once a word and a sentence: it is a word inasmuch as it refers to this-house-on-fire or firing-of-those-guns, and it is a sentence inasmuch as a listener or several listeners are involved in the act of communication or command. Such one-word sentences are now admitted by all modern grammarians, and it is not to be supposed that any objection will be raised on this score[1].

A conceivable objection might, however, be found in the utterance of the word 'you,' which, when it occurs out of context, is certainly felt as a mere word and not as a sentence. And yet there is here, always present, an attitude of the speaker towards a listener. But my definition said 'volitional attitude' and not simply 'attitude.' *You, your, yours* and sometimes *our, ours* contain a reference to the listener as whole or part of what these words speak of; just for this reason they are words, but they are not sentences until they imply in addition a volitional attitude on the part of the speaker. If the question is put 'whom will Tom send?' and the answer is 'you,' then 'you' is a word by virtue of the fact that it refers to a thing spoken about, in this case the listener, but it is now also a sentence because the articulation conveys the information to the listener that it is he whom Tom will send. The same argumentation will apply to 'yours' as answer to the question 'whose pen is this?'

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[1] See P. Wegener, *Der Wortsatz* in *Indogermanische Forschungen*, 39 (1920), 1–26. In no book that I have consulted is there shown a better understanding of the true nature of Language than in the same author's *Untersuchungen über einige Grundfragen des Sprachlebens*, Halle, 1895.

Such exclamations as 'alas,' 'hurrah' or 'damn' may perhaps be quoted as objections. We feel them as self-sufficient and accordingly, it must be supposed, as sentences; sometimes, too, it will be admitted, they make a deliberate appeal to the sympathy of a listener, and so come well within my definition. For my own part, I am prepared to concede, however, that such exclamations might possibly come to the lips even in solitude. Now in such a case, I think, it is for the psychologist to decide whether there is any dual function on the part of the speaker causing him to act as listener as well as speaker. In articulate remarks addressed to oneself, such as sometimes undoubtedly occur, a speaker actually does seem to be acting a double rôle; if a man says to himself "You must do such and such a thing," this is an autosuggestion modelled on the form usually assumed by the suggestions of others; the coercive, imperative tone is borrowed from Language as a social phenomenon, and we are here dealing with a form of Language which is clearly secondary. The same kind of answer may be made if it is objected that the lyrical poet writes only for himself, though undeniably he writes in sentences. Sir Walter Raleigh in his essay on Style comments as follows: "Yet it were rash to say that the poets need no audience; the loneliest have promised themselves a tardy recognition. The poet seems to be soliloquizing because he is addressing himself, with the most entire confidence, to the small company of his friends, who may even, in unhappy seasons, prove to be the creatures of his imagination." Hardly less than poetry, many books of science and philosophy might at first sight seem to lay claim to emancipation from an audience; their sentences seem to state what is, and writer and audience are, to say the least, in eclipse. But when we look closer, the attitude towards a circle of readers is seldom far from the surface; no book is written without some desire to instruct, to convince, or to place on record for oneself or for others.

How far sentences, and not merely disjointed words, occur in silent thought is a question of deep interest, to which it is to be hoped that psychology will give us a detailed answer. So far as my own limited and unpractised introspection goes, though I am very conscious of verbal elements in ordinary thinking, yet it is only when I have some writing in view or when I wish to fix some conclusion in my memory that I am aware of framing complete unspoken sentences. If we were in the habit of thinking in finished sentences, surely the difficulty which is often found in formulating a thought would not be experienced at all. In the cases to which I have referred, it may, moreover, be contended that there is a latent audience. But—and this is the important point—even if such is not the fact (the answer rests with psychological experiment) my definition of the sentence will not be invalidated. For of course it is possible for the sentence, as for almost anything, to be transferred to a usage for which it was not originally intended and where it ceases to be what it was designed to be except in the historical, evolutionary sense. Such a transferred use of the sentence is seen in what we are wont to call subordinate sentences, which, as every grammarian knows, are merely sentence-like equivalents of words, whether nouns, adjectives or adverbs. It is established that many subordinate sentences did actually originate in real sentences, real assertions, as for example noun-clauses of the type 'I see that he is content' from 'I see that: he is content.' How little of the true nature of a sentence there is in what we call subordinate sentences is well exhibited in such a case as 'that he has gone proves that he does not care,' where if we lop away the two so-called subordinate sentences there remains for the main sentence only the word 'proves,' which in its isolation no rational person will admit to be a sentence at all. Jespersen and others have pointed out the need for a radical reform in our terminology here[1].

On my definition the vocative will have to be a word-sentence, since it both names the listener and also invites his attention. Otherwise no alteration whatsoever will have to be made, so far as I can see, in our ordinary classification.

There is, however, just one objection to which I can give no quite satisfactory answer, though I fancy it is not of much importance. Occasionally a mere word may indicate an attitude of respect or disrespect towards the listener, and if the choice of that word be intended to please or offend, it is difficult to avoid the conclusion that on my definition the word is then a sentence. Thus when a Frenchman, in addressing a close kinsman or intimate friend, suddenly replaces the pronoun *tu* with *vous*, this *vous* may often be practically equivalent to the sentence 'I am very angry with you.' Such overtones of meaning are perhaps the most powerful weapon that speech possesses. Meredith illustrates this point with consummate skill in a passage of *Rhoda*

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[1] Jespersen, *Sprogets Logik*, 38–9. So, too, Paul, *op. cit.*, 296–7. *The Report of the Joint Committee on Grammatical Terminology*, p. 14, likewise recognizes the difficulty, for it recommends that in cases where a complex sentence "contains a subject and predicate of its own it be called the *Main Clause*, but that where it does not, it be called the *Main Predicate* or the *Main Clause*, as the case may require."

Fleming[1], where the literal meaning of the spoken words sinks into insignificance beside the emotional values (added in brackets by the author) with which they are pregnant.

> "Then he perceived in dimmest fashion that possibly a chance had come to ripeness, withered, and fallen, within the last scoffing seconds of time. Enraged at his blindness, and careful, lest he had wrongly guessed, not to expose his regret (the man was a lover), he remarked, both truthfully and hypocritically, 'I've always thought you were born to be a lady.' (You had that ambition, young madam.)
>
> She answered: 'That's what I don't understand.' (Your saying it, O my friend!)
>
> 'You will soon take to your new duties.' (You have small objection to them even now.)
>
> 'Yes, or my life won't be worth much.' (Know, that you are driving me to it.)
>
> 'And I wish you happiness, Rhoda.' (You are madly imperilling the prospect thereof.)
>
> To each of them the second meaning stood shadowy behind the utterances. And further,—
>
> 'Thank you, Robert.' (I shall have to thank you for the issue.)
>
> 'Now it's time to part.' (Do you not see that there's a danger for me in remaining?)
>
> 'Good-night.' (Behold, I am submissive.)
>
> 'Good-night, Rhoda.' (You were the first to give the signal of parting.)
>
> 'Good-night.' (I am simply submissive.)
>
> 'Why not my name? Are you hurt with me?'
>
> Rhoda choked. The indirectness of speech had been a shelter to her, permitting her to hint at more than she dared clothe in words.
>
> Again the delicious rose glowed between his eyes.
>
> But he had put his hand out to her, and she had not taken it.
>
> 'What have I done to offend you? I really don't know, Rhoda.'
>
> 'Nothing.' The flower had closed."

The discussion of these obscurer purposes of Language would lead us too far afield, and my object in the present paper is not to consider any new meanings to which the terms 'word' or 'sentence' might be extended, but rather to analyse the little comprehended but universally felt senses in which they are familiar to us. Let me state my definitions once again:

A Word is an articulate sound-symbol in its aspect of denoting something which is spoken about.

A Sentence is an articulate sound-symbol in its aspect of embodying some volitional attitude of the speaker towards the listener.

It is evident from these definitions that the terms 'word' and 'sentence,' abstractly considered, are quite heterogeneous. The common notion that a word is necessarily part of a sentence, and that this is all the difference which exists between the two, breaks down in the case of one-word sentences, where the word and the sentence are concretely coextensive. Doubtless the reluctance which students formerly felt in allowing the existence of one-word sentences was due to the mistaken idea that if it be admitted that a word can be at the same time a sentence, the very useful distinction between the two disappears. Such, however, as I have tried to point out, is not the case; the terms 'word' and 'sentence' refer to totally different aspects of concrete linguistic phenomena, and surely it is permitted to call a rat a rodent from one point of view, and a nuisance from another. Still, the fact remains to be explained that in concrete reality no sentence exists which is not at the same time either a word or else composed of words. The reason is that we deliberately restrict the term 'sentence' to purely linguistic phenomena, and that we rule out from the field of linguistic phenomena everything that is not an articulate sound-symbol having reference to a thing, *i.e.* a word. Otherwise we might easily extend the meaning of the term 'sentence' to many other social gestures, to an imploring look or to a shake of the fist. If we did this, the sentence would cease to be a purely linguistic term, and its essential heterogeneity as contrasted with the word would become quite apparent.

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[1] London: Constable & Co., 1902, 379–80.

One last thought is suggested by the foregoing discussion. At what angle is the critic of Language to place himself? Is the meaning of a sentence that which is in the mind of the speaker at the moment of utterance, or that which is in the mind of the listener at the moment of audition? Neither, I think. Certainly not that which is in the mind of the listener, for he may utterly misconstrue the speaker's purpose. But also not that which is in the mind of the speaker, for he may intentionally veil in his utterance the thoughts which are in his brain, and this of course he could not do if the meaning of the utterance were precisely that which he held in his brain. Obviously we must postulate for the critic a position which ignores neither the speaker nor the listener, nor yet again the words that pass between them. I think that the following formulation will meet the case: *The meaning of any sentence is what the speaker intends to be understood from it by the listener.*

Postscript.

The article in *Indogermanisches Jahrbuch* VI (1918), 1–20, referred to above (p. 353) has now come to hand, and proves to be a well-written and thoughtful discussion, by Karl Bühler, of prevailing opinions on the sentence. The writer starts from the view that speech has three functions, namely *Kundgabe, Auslösung* and *Darstellung*, *i.e.* approximately 'self-expression,' 'demand for response' and 'description.' It is shown that none of these functions alone yields a satisfactory definition of the sentence, though this must necessarily be defined, not in reference to its mode of origination, but in reference to its purpose. Bühler therefore finds the essence of the sentence to lie in its possession of what is common to the three functions of speech, namely *Sprachzweck* ('purpose') or *Sinn* ('sense'). The former term, when analysed further, leads, as I believe, directly to my 'volitional attitude of speaker to listener,' though Bühler does not see this, failing to recognize the absolute indispensability of the listener to language-theory[1]. The use of the term *Sinn* is decidedly a step in the wrong direction, being even less clear than *Sprachzweck*, and leading to a hair-splitting distinction between *Sinn* 'sense' as the characteristic of the sentence, and *Bedeutung* 'meaning' as that of the word (p. 18). Bühler does not realize that each of his 'functions' enters into *every* genuine linguistic utterance, *i.e.* employment of speech as a social instrument. Every sentence is *Kundgabe* (roughly 'interjectional') inasmuch as it expresses what the speaker has to say, *Auslösung* (roughly 'imperative') inasmuch as it claims, at the very least, the attention of the listener, and *Darstellung* (roughly 'assertive') inasmuch as something or other is always spoken about. Each of these three functions may come into the foreground as the specific purpose of a given utterance, and from this variation arise the different types of sentence. In conclusion, I venture to recommend Bühler's paper as a stimulating and valuable contribution to the topic; I think he has failed to solve the problem, but he has seen many of the difficulties and pointed in the right direction.

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[1] On p. 13 Bühler falls into the common error of regarding the use of language in solitary meditation as a proper use of language as such. I must insist once again on my contention that, however important this use of language may be and is, nevertheless it is secondary and a by-product—as much so, as when a book is used as a paper-weight. No progress can be made with the theory of language if the latter be removed from its proper sphere, namely social intercourse, where for its successful functioning a sympathetic interaction of speaker and listener is demanded, besides the existence of a world and of a tongue common to both.

A VINDICATION OF THE RESONANCE HYPOTHESIS OF AUDITION. III[1].

By H. HARTRIDGE.

(From the Cambridge Physiological Laboratory.)

1. *The lengths, masses and tensions of the fibres of the organ of Corti* (pp. 364–367).
2. *Physical requirements of the resonance theory* (pp. 367, 368).
3. *Physical requirements if the auditory nerve obeys the all-or-none law* (pp. 368–371).
4. *The number of basilar fibres is sufficient to account for experimental results* (p. 371).
5. *The inter-connexion of fibres of basilar membrane offers no difficulty* (p. 372).
6. *Such inter-connexion must add to the efficiency of the ear* (pp. 372, 373).
7. *The perception of tones of different degrees of loudness is readily explained* (pp. 373, 374).
8. *The neurology of audition* (pp. 374, 375).
9. *Developmental evidence* (p. 375).
10. *Tones not recognised by number of nervous stimuli per second reaching the brain* (pp. 375, 376).
11. *Ewald's theory is not unique in having been tested by means of a model* (p. 376).
12. *The existence of a constant tension in the fibres of the basilar membrane is not contrary to biological laws* (pp. 376, 377).
13. *The advantages of filling the cochlea with liquid* (pp. 377–379).
14. *Pitch limits* (pp. 379, 380).
15. *Summary* (pp. 381, 382).

In this paper I propose to deal (*a*) with the essential part of the Helmholtz theory which concerns the resonators and their properties, and (*b*) with certain criticisms which have been advanced against this part of the theory. It is now universally agreed that the sound wave motion of the air is in greater part transmitted to the cochlea *viâ* the chain of ossicles in the middle ear. It is also agreed that the hair-cells in the cochlea are the sensory end organs alone concerned with the origination of auditory stimuli received by the brain. Differences of opinion still exist as to the way in which motion of the piston of the stapes is communicated to these hair-cells. However, the debatable points have been reduced considerably of recent years, and I believe that the weight of opinion may be classified as follows:

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[1] The two previous papers on audition have appeared in this *Journal*, 1921, XI, 277–288; XII, 142–146.

(1) The belief that the basilar membrane moves as a whole. Rutherford, Wrightson and Keith.

(2) The belief that the basilar membrane is set in motion selectively, according to the agreement between the period of the incoming sound waves, and the natural rate of vibration of the different basilar fibres. Helmholtz, M'Kendrick and Gray.

(3) The belief that the tectorial membrane contains the resonating structures and not the basilar membrane. Hardesty and Luciani.

(4) The belief that the basilar membrane is selectively set in motion but not by resonance. M. Meyer and Ewald.

It will be observed that opinions 1, 2 and 4 have this in common, that they hold that the basilar membrane is the structure responsible for the stimulation of the hair-cells. In my opinion the evidence for this is so weighty that any alternative hypothesis needs no further consideration, *i.e.* I rule out belief 3 above as impossible. In this connexion the reader should turn to Keith's description, pp. 176–186 of Wrightson's book[1].

In some details Helmholtz's ideas were almost certainly defective. For example, he believed that the tips of the hair-cells are thrust against the under side of the tectorial membrane, each time the basilar membrane rises; but it is much more likely that, as Keith states, the tips of the hairs are implanted into the under side of the tectorial membrane, and Keith holds that they undergo bending twice during each complete cycle.

While agreeing with Keith that the anatomical evidence is in favour of the tips of the hairs being implanted in the under side of the tectorius, yet it seems to me that there is another manner in which the stimulus to the hairs might occur. I believe with Keith that the one fulcrum of the basilar membrane is close to the external spiral groove, but it seems to me that the other fulcrum is situated not where the internal spiral ligament begins (as Keith and Wrightson suppose), nor where it is attached to the bony spiral lamina, but near some point between the two.

The tectorius will, if this is the case, not lie over one edge of the vibrating structures but over approximately their centre. The drawing by Fraser and Dickie (Fig. 23, p. 235 of Wrightson's book) shows this feature not only for fibres of medium length, but also for those at the top and the bottom of the cochlea. If then the tectorius is attached nearly at the centre of the vibrating mass, it will be nearly at that part which undergoes the greatest movement during vibration. When therefore the basilar membrane moves upwards the tectorius would be urged upwards too, but when the basilar membrane reaches the end of its motion and returns, the tectorius, possessing mass and therefore momentum, would tend to go on rising and would therefore pull on the processes of the hair-cells with the force needed to give to the tectorius that acceleration which will cause it to follow the down motion of the basilar membrane. According to this view the stimulus to the hair-cells is not by pressure (Helmholtz), nor by friction (Wrightson), nor by bending (Keith), but by traction (Hartridge). One advantage of this view is that (*a*) it explains why the stalk of the tectorius and the internal spiral groove widen as the basilar membrane widens, *i.e.* in order that the tectorius shall lie over the centre of the moving basilar membrane; (*b*) it explains why the tectorius being "inconceivably delicate and flexible" (Hardesty, Keith and Wrightson) is yet able by its mass to stimulate the hair-cells (it cannot, I think, be rigid enough to act as a bent lever, as Wrightson's and Keith's views would require); (*c*) it gives a better explanation of the mode of stimulus in the bird and alligator in which the supposed side-ways motion of the tectorius over the hair-cells is not nearly so readily visualisable as in mammals.

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[1] Wrightson, *The analytical mechanism of the internal ear*, 1918.

We have now to consider the evidence for and against the resonance of the basilar membrane.

The evidence in fàvour is that (*A*) the experiments described in my previous paper[1] show that there are resonators somewhere; these cannot be situated in the external or middle ear, or in the brain, and must, therefore, by exclusion be looked for in the internal ear; (*B*) inasmuch as the structure which receives motion from the piston of the stapes *vià* the perilymph of the scala vestibuli and communicates that motion to the hair-cells at each cycle, is the basilar membrane, therefore the basilar membrane, with its arches, hair-cells, etc., must form the system of resonators, as Helmholtz concluded[2].

Against this hypothesis the following objections have been advanced.

1.

Keith objects[3] that the audible range of 40 to 40,000 vibrations per second (see section 14) could not be received by resonators varying in length according to him from ·12–·30 mm. and Roughton and Thacker[4] have objected that even if mass and tension as well as length are taken into account, as I have previously suggested[5], the wide range of tones perceived by the ear could not be accounted for, because the low-tone fibres would have to be so lightly stretched that the basilar membrane would sag, whereas on the other hand the high-tone fibres would have to be so tightly stretched that they would break.

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[1] Hartridge, this *Journal*, 1921, XII, 142–146.

[2] Helmholtz, *Sensations of Tone*. Ellis's translation, 1912, 145.

[3] *Op. cit.* 174.

[4] In a private communication to the author.

[5] Hartridge, this *Journal*, 1921, XI, 283.

Assuming that the fibres of the basilar membrane do not differ markedly from silk in their physical properties, I made measurements to see (*a*) what is the breaking strain, and (*b*) what is the least tension that would give vibrations without sag. The first was found to be 1200 grams and the second ·05 gram approximately. The least and greatest lengths for the basilar fibres as given by different observers are:

Observer	Greatest	Least	Ratio
Keith	·300	·120	2·5 to 1
Retzius	·240	·140	1·7 to 1
Henle	·410	·023	17·8 to 1
Hensen*	·370*	·030*	12·5 to 1

* Inferred.

It will be seen that there is a big divergence of opinion, which histologists should endeavour to bridge by further measurements. Taking Keith's values of ·12 and ·30 mm. respectively for the lengths of the highest and lowest resonators and 40,000 and 40 for their number of vibrations per sec., then putting X and Y for the weights in the two cases as found by experiment above, and substituting in the ordinary formula for a stretched string[1], viz.

$$R=\frac{1}{2L}\sqrt{\frac{T}{M}},$$

we find that $X = 1{\cdot}27$ grams per cm., $Y = 8{\cdot}55$ grams per cm., the ratio between the two being 1 to 6·7. Since the mass per unit length varies as the square of the linear dimensions, the latter should be in the ratio of 1 to 2·6, a figure which is well within the range of possibility. If, instead of Keith's values for the lengths of the basilar fibres, Hensen's are employed, then the conditions are much more favourable, for no change in mass per unit length whatever would be required.

A still more convincing answer to this objection is obtained, as Roughton has pointed out, when we make an approximate calculation of the actual tension per sq. cm., which the high-tone and the low-tone fibres would have respectively to possess in order to function efficiently as resonators.

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[1] Poynting and Thomson, *Sound*, 1904, 88.

The usual formula $R = \frac{1}{2L}\sqrt{\frac{T}{M}}$ can be altered to $R = \frac{1}{2L}\sqrt{\frac{T}{D \times A}}$, where D is the density, M is the mass per unit length, and A is the section area because $M = D \times A$.

Now since $D = 1$, approximately, $\frac{T}{A} = 4R^2 \times L^2$. Taking the values of R and L for the shortest resonator, viz. $R = 40{,}000$ and $L = \cdot 12$ mm. (Keith's value), we find that $\frac{T}{A} = 920{,}000$ dynes per sq. cm. But this value is only a negligibly small fraction of the stress which animal fibres have been observed to bear without breaking, as shown in the following table:

Silk[1]	...	26,000,000 grams per cm.
Hair	...	6,600,000 ,, ,,
Tendon	...	2,300,000 ,, ,,

Considering similarly the tension of the longest fibres and taking 40 as the natural period and ·3 mm. as the length, the tension per sq. cm. works out at about 6 dynes, the value being very low as would be expected. This disposes of Perrett's criticism[2] that "the tension of a string $\frac{1}{50}$ of an inch long and of microscopical thickness, having a periodic time of $\frac{1}{33}$ of a second, must, of course, by Brook Taylor's formula (1715), be something enormous!"

But a further question arises. Is such a very low value for the tension sufficient to prevent the fibre from sagging? That the answer can be 'yes' seems at first sight very doubtful, since we find, by a simple calculation from the experiments quoted above, that the minimum tension which will just prevent a horizontal silk fibre from sagging is as high as 1100 grams per sq. cm. This is about 80,000 times greater than the value we have just arrived at for the tension of the low-tone fibres. But it must be remembered that the minimum tension necessary to prevent sagging will obviously vary directly with the effective weight of the fibre. The effective weight will in its turn vary directly with the difference between the density of the fibre and the density of the medium in which it is immersed. Since the density of a basilar fibre presumably differs exceedingly little from the density of the fluid in the cochlea, the effective weight of a basilar fibre *in situ* must be enormously less than the effective weight of a silk fibre in air. Correspondingly, therefore, the minimum tension necessary to prevent sagging must also be enormously less in the former case than in the latter.

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[1] Kaye and Laby, *Physical and Chemical constants*, 28.

[2] Perrett, *Some questions of phonetic theory*, 1919, chap. v, p. 10.

The suggestion that the fibres of the basilar membrane are tightly stretched near the base of the cochlea and *vice versâ* is confirmed by Keith (*op. cit.* p. 237). He states that the lowest bridge with the shortest span is provided with massive lateral supports whereas the top-most with more than twice the span has the weakest.

The conclusion reached is, I think, that with the data at our disposal the Helmholtz hypothesis fully justifies us in thinking that the physical properties of the resonators could give approximately the range of tones met with in the human ear.

2.

Keith[1] has objected that "There are no anatomical structures that can serve as resonators in the cochlea." "The basilar fibres are not free strings, they are clothed on their lower surface by a layer of loose cells and fibres, and their upper surface is covered by a stratum of cells."

When Helmholtz advanced the view that the internal ear contained resonators, he knew that they would have to be different in their properties from the resonators handled in the physical laboratory. The resonators of the laboratory are of two types:

(*A*) Those, like tuning forks and tightly stretched strings, which have very sharp tuning, *i.e.* they do not vibrate unless the air waves setting them in motion have almost precisely the same pitch as themselves. But resonators of this type when once they are set into vibration, continue in motion for a considerable time after the stimulating sound waves have ceased, and only gradually do they die away to rest, *i.e.* their 'persistence' is very great.

(*B*) Those, like the receiver of the telephone, or the sound box of the gramophone, which are designed to respond as equally as possible to all incoming sounds whatever their pitch, and being dead beat come to rest immediately the sound waves cease.

Now the ideal requirements of the ear would be to have resonators sharply tuned like the (*A*) class, and coming to rest at once like the (*B*) class. But these two properties are physically antagonistic, no resonator can possess both properties to a high degree at once. Therefore, as Helmholtz saw, the ear resonators must be a compromise between classes (*A*) and (*B*)—a type of resonator in fact seldom seen in the physical laboratory.

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[1] Wrightson, *loc. cit.* 187.

He investigated the possible properties of various compromise resonators (*V*—*Z*) and found that they could be stated by a table[1] as follows:

TABLE I.

TYPE	TUNING	PERSISTENCE
(*A*) Stretched string	Very sharp	Very great
V	⅛ tone	38 vibrations
W	¼ ,,	19 ,,
X	½ ,,	9·5 ,,
Y	1 ,,	4·75 ,,
Z	2 tones	2·37 ,,
(*B*) Telephone	Very small	Very little

As the result of experiments, performed by Helmholtz to determine which type in the above table is to be found in the human ear, he decided that type *X* was the one found for middle scale notes, inclining towards type *W* for high notes and to type *Y* for low notes. Now it is clear that resonators of this type could not consist of stretched cords alone, or they would conform too much to type *A*; similarly, they could not consist of layers of living cells alone, or they would conform too much to type *B*. In order to have the necessary physical properties they must consist both of stretched strings and of layers of cells. We see therefore one reason why it is necessary for the basilar fibres to be clothed above and below by living cells[2].

3.

Roughton has also objected that the number of resonators is insufficient particularly if the cochlea nerve obeys the 'all-or-none' law[3].

Helmholtz concluded as the result of experiment that type *X* resonator is the one found to respond to musical tones of medium pitch. He calculated[4] that if a certain musical note exactly in tune with one such resonator caused it to vibrate with an amplitude of 100, then other notes of the same loudness would cause the following amplitudes (Table II).

Suppose now that there are two resonators in the ear differing from one another by ·10 of a tone, and that a musical note just between the two be sounded, then it is clear that both resonators will respond with a 93 degree amplitude of vibration. If, now, the musical note be made sharper by ·01 of a tone, then the vibrations of the shorter resonator will increase

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[1] Cf. Helmholtz, *op. cit.* 143.

[2] Another reason is probably to nourish and protect the basilar fibres, in much the same manner as Bowman's and Descemet's elastic layers in the cornea are covered by a layer of living cells.

[3] Communicated privately to the author.

[4] Cf. Helmholtz, *op. cit.* 145.

TABLE II.

Difference in pitch between resonator and note	Amplitude of vibration of resonator
·00 of a tone	100 degrees
·01 ,,	99·5 ,,
·02 ,,	98·5 ,,
·03 ,,	97 ,,
·04 ,,	95·5 ,,
·05 ,,	93 ,,
·06 ,,	90·5 ,,
·07 ,,	87·5 ,,
·08 ,,	84 ,,
·09 ,,	79 ,,
·10 ,,	74 ,,

in amplitude from 93 to 95·5 and those of the longer decrease from 93 to 90·5. The shorter resonator will now have nearly 5 per cent. greater amplitude of swing than that of the longer, and this difference should readily affect the auditory brain centres *viâ* the cochlea nerve. Suppose now that the musical note be made sharper by one-fifth the above, *i.e.* by ·002 of a tone. Then the two resonators instead of both vibrating with an amplitude of 93 will now have amplitudes of 93·5 and 92·5, *i.e.* there will be a difference of 1 per cent. Now the sense organs of the skin and the retina respond to differences in stimulus strength of about 1 per cent. so that such a difference in the amplitudes of the ear resonators should be readily detected if the cochlea nerve does not obey the 'all-or-none' law, *i.e.* is graded in its response. Now Auerbach stated that the trained ear can perceive a difference of ·25 vibration per second over the greater part of the musical scale, *i.e.* about 2000 distinguishable pitches in the octave between tones 500 and 1000 vibrations per second, *i.e.* ·003 of a tone. Preyer gave about half that value, viz. about 1000 distinguishable pitches per octave, *i.e.* ·006 of a tone. Delezenne gave the following values, at a tone of 120 vibrations per second, a ·03 of a tone, at 500 vibrations ·005 tone, at 1000 vibrations ·0042 tone. Külpe gives at 32 vibrations ·1 tone, at 260 vibrations ·0062 tone, and at 2048 vibrations ·0012 tone. The average value found by the four observers for tones between 260 and 2048 vibrations per second is ·0042 of a tone as the least perceptible difference. Now the difference in pitch taken for purposes of comparison above, viz. ·002 of a tone, is less than half the average value found by experiment, and we have seen that resonators differing in natural frequency by ·1 of a tone should readily perceive this. But as a matter of fact, the resonators in the human ear, as will be shown later, differ by about ·01 of a tone, *i.e.* there are ten times as many resonators as I have assumed above; that is, one-tenth of the number actually present would still suffice to give the requisite acuity. The number of resonators present is therefore amply sufficient to meet the needs of the ear.

Now it has been assumed in the above reasoning that the auditory brain centre receives information not only that a certain resonator is vibrating, but also that the amplitude of that vibration has a certain value, *i.e.* the nervous impulse is graded. The possibility has to be kept in mind, however, that sensory nerves like motor nerves may obey the 'all-or-none' law. There is no evidence at the present time either for or against this view, but Troland[1] has recently considered the 'all-or-none' law from the point of view of vision, so that a similar consideration in the case of hearing may be advisable The maximum number of separate resonators in the human ear is, according to Keith, about 4000 since this is the number of complete arches of Corti (Waldeyer gives the number as 4500). Each arch has about four hair-cells (one inner and three outer) connected to it and each of these cells has its own nerve fibre. Each arch rests on approximately five basilar membrane fibres. Now the musical scale may be said to extend from 64 to 4096[2], *i.e.* over six octaves; if we assume that 600 resonators are devoted to each of these octaves, about 3600 resonators will be accounted for, leaving some 400 resonators to deal with the lower limit from 16 to 64, and the upper from 4096 to 40,000 (a more than sufficient number, for M'Kendrick says 110 would suffice). Since there are six tones in each octave, and each octave is represented by 600 resonators, the resonators must differ from one another on the average by ·01 of a tone. (This value is intended to be the average one throughout the musical scale, and is probably too good a value for the ends, but too poor a value for the middle.) If we assume that each hair-cell and its nerve fibre obeys the 'all-or-none' law, and that as the amplitude of vibration is increased, first one cell and then another begins to receive its adequate stimulus, then any one resonator can respond in five stages: (1) not vibrating; (2) one cell responding; (3) two cells; (4) three cells; (5) all cells. Suppose then that we sound a tone strictly in tune with one of the resonators, a large number of other resonators on either side will also be set into vibration but with diminished amplitude —according to the values of Table II above. If now the tone sounded be made sharper or flatter by ·0025 of a tone, between about 1 and 2 per cent. of these resonators will suffer a change, there being an increase in the number of hair-cells stimulated in the case of some and a decrease in the case of others. This change of 1 to 2 per cent. should be readily detected, and a difference of ·002 of a tone should therefore be appreciated. Now the mean value for the differential threshold for the discrimination of pitch given above was ·0042 of a tone. There are therefore ample resonators in the internal ear to satisfy experimental requirements even if the ear obeys the 'all-or-none' law. Lastly, if we assume that the four hair-cells of any one resonator operate together, and not separately as assumed above, then to perceive a total of 11,064 different pitches (which is M'Kendrick's estimate) there should be 5532 separate resonators and not 4000 as Keith found, or 4500 as found by Waldeyer. To overthrow Helmholtz's theory, however, it would be necessary to count the number of different pitches and the number of resonators in the case of one and the same man, and to demonstrate that, beyond experimental error, the former was more than twice as large as the latter.

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[1] Troland, *Journ. Amer. Opth. Soc.* 1920, 161.

[2] M'Kendrick. Schäfer, *Text-book of Physiology*, II, 1184.

4.

It may be asked[1], why,—if as Helmholtz supposed a considerable number of hair-cells are stimulated even by a single pure tone, but with different intensities (the actual in-tune vibrator stimulating its hair-cells most and others less according to their difference in tuning)—will not relatively few resonators suffice to provide the necessary acuity for difference of pitch that the ear is found to possess?

To put this criticism in a more concrete form, we have seen in the previous section that if we assume that the auditory nerves can convey a graduated impression, then the tone difference acuity of the ear could be provided by resonators differing from one another in their free periods by ·1 of a tone. Actually, however, they differ by one-tenth of this amount, *i.e.* they are ten times more numerous than they apparently need be, why is this?

The following explanation may be given. Although, by careful attention, the ear could work with fewer resonators, yet for a rapid and accurate estimate of pitch a greater number has been found necessary. If the auditory nerve obeys the 'all-or-none' law, then again rather fewer resonators would suffice, but still a greater number would be advisable for the reasons just given. If the spatial acuity of the eye was reduced to one quarter by a reduction in the number of retinal elements, we should still be able to see sufficiently well for practical purposes, but the observations would take longer, would require greater attention and would be more liable to error than they are.

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[1] The actual question was put to me by Dr H. Lamb.

5.

Wrightson[1] has stated that one of the greatest difficulties encountered in the Helmholtz theory of resonance in the fibres of the basilar membrane is that these fibres are not free but connected sideways. If the figures calculated by Helmholtz for the resonators of the cochlea in Table II be again considered, it will be observed that with a difference of ·2 of a tone between the free period of a resonator and that of the tone setting it in vibration, the amplitude will be 40 per cent. of that of the maximum possible. Now since the average difference between vibrators has the value of ·01 of a tone, it follows that 20 vibrators must lie between that with the maximum and that with 40 per cent. amplitude; that is, that between any two resonators there will be a difference of amplitude on the average of about 3 per cent. Even if all the resonators were free this value states the average difference in amplitude between the swing of two neighbouring resonators that the Helmholtz theory would require. The table shows further that the greatest difference between the amplitudes of any two neighbouring resonators is under 6 per cent. Can the different resonators be so tightly bound together that this difference in amplitude is not possible? I do not know of any evidence that such is the case. If any critic of Helmholtz should retort that there is evidence of this, I would refer him to Wrightson's book, pp. 234–8. Here Keith says: "the power of the basilar membrane to resist pressure diminishes as the membrane is followed from base to apex of the cochlea." "The bridges become more yielding, with greater amplitudes of swing as we ascend from base to apex." "From base to apex the cochlea is designed to deal with displacements which vary in amount and rapidity." Can this be the case if the whole of the basilar membrane is so firmly bound together that individual movement to the extent of 6 per cent. is impossible? In the next section an explanation will be found for this joining up of the basilar fibres to form a continuous membrane.

6.

Hensen discovered that certain tones set some hairs of the opossum shrimp into vibration, and other tones other hairs. If a system of resonators can be established so simply, an anatomist may well wonder why so complex a structure as the organ of Corti should have been elaborated for this purpose[2].

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[1] Wrightson, *op. cit.* 104.

[2] Keith in Wrightson, *op. cit.* 188.

The answer to this criticism can best be met by considering the case of two tuning forks of the same pitch, one of which is driven mechanically, the other being set into vibration by resonance through the medium of the intervening air. As is well known, the amplitude of the second fork will be only a small fraction of that of the first, even if the length of time during which resonance is allowed to occur is long. But if now the prongs of the forks could be connected to pistons and these pistons be fitted one into each end of a tube containing air, then the amplitude of the driven fork would be more nearly equal to that of the driving one. If further the connecting tube were to be filled with water instead of air, then once more an increase in amplitude would be found to occur. Now, as Keith has pointed out[1], it is the purpose of the ear not only to provide man with the enjoyment of music but also to respond to the slightest disturbance, and so warn the lower animals of danger. To do this, the motions of the piston of the stapes must be conveyed to the hair-cells with the least possible loss. Obviously this could not be effected if the hairs were free as in the opossum shrimp or if the fibres of the basilar membrane were isolated one from the other, but could be effected if all the basilar fibres were joined together so that there was no leak of fluid between them. Such an arrangement does not, as I have shown in the previous section, prevent them resonating separately, but does cause them to utilise all the available energy. This, I am convinced, is the explanation of a feature of the organ of Corti which, more than any other, has strangely proved an apparent stumbling-block to the resonance theory.

7.

The following question has been asked[2], that if we assume that the auditory nerve obeys the 'all-or-none' principle, how can we explain the perception of notes of different loudness? According to the estimate given above, there are approximately 4000 complete resonators in the human cochlea. If this number is distributed over the range of audition it will be found that near the middle of the musical scale each resonator will differ from its neighbour by about ·01 of a tone. Now Helmholtz found by experiment that roughly ten vibrations must be performed at the end of a musical note before any one resonator will be reduced to one-tenth of its amplitude, and that such a resonator will vibrate with one-tenth the full amplitude when a note a semitone sharp or flat is sounded. Since each resonator differs from its neighbour by ·01 of a tone,

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[1] Keith in Wrightson, *op. cit.* 237.

[2] This question was put to the author by Mr Barcroft and Mr Roughton.

50 resonators above and 50 resonators below the in-tune resonator will be vibrating with more than 10 per cent. of the maximum amplitude; and, therefore, if we assume, as above, that a vibration of more than 10 per cent. is required for the auditory nerve to respond, then, since each resonator having four hair-cells can respond in four different intensities, such a note could be produced in approximately 400 different degrees of diminishing loudness. The suggestion that a pure tone can stimulate 100 or more resonators should not cause difficulty any more than that the head of a pin can stimulate 100 touch corpuscles.

So far as increasing loudness is concerned, the limit would seem to be reached only when neighbouring notes begin to overlap in their effects on the intervening resonators, or when dissonant resonators stimulate the auditory nerve. In this connexion the noisiness of very loud musical tones should be referred to. This limit would appear to have been reached in the case just considered, and therefore 400 different degrees of loudness is approximately the limiting number available according to theory. It would seem that this number would more than satisfy the demands of experience. If, on the other hand, the four hair-cells respond together or have a common nerve fibre the number of degrees of loudness would be reduced to 100. But this number even would seem to be sufficient to satisfy the conditions.

8.

Kolmer found that one single fibre of the auditory nerve may, by branching, supply two separate hair-cells. Luciani has stated that this directly refutes Helmholtz's theory. On the other hand, Held found that a single (nerve) fibre may be distributed along a quarter turn of the cochlea including a series of some 300–400 arch segments.

It will be observed that not only do Kolmer and Held contradict one another, but also that both, so far as the ear of man is concerned, are contradicted by the following facts: Snodgrass[1] found that in an adult with normal hearing there were approximately 14,000 fibres in the cochleal division of the auditory nerve. Keith[2] gives the number of fibres as 15,000. Each of these fibres must have had a ganglion cell, and from each cell a single process must run radially outwards towards the hair-cells. Now the hair-cells in man number 15,237 in all[3], so that practically each hair-cell has its own separate nerve, and very few (474, about 3 per cent.) would share a nerve fibre with another cell. This disposes of Held's evidence, for, according to his statement, the 3½ turns of the cochlea would be supplied by not more than 14 nerve fibres in all. The ear that he examined, if it was human, must have been highly abnormal. The possibility that the two hair-cells are fed by one and the same cochlea nerve fibre does not embarrass the resonance theory. For histological evidence shows that there are four hair-cells (one inner and three outer) in structural association with each complete arch of the organ of Corti. Since each arch forms part of the essential resonating element, all four cells belonging to an arch could, for the purposes of the resonance theory, be connected to one and the same nerve fibre. But even if it were subsequently found that in certain cases more than four cells were so connected, or cells belonging to different arches, it would only mean that that part of the ear was less efficient than it might be, in much the same way that the rods in the periphery of the retina being connected in groups with one optic nerve fibre, give a lower visual acuity than the cones of the fovea which have a nerve fibre apiece.

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[1] M'Kendrick in Schäfer's *Text-book of Physiology*, II, 1184.

[2] Keith in Wrightson's book, p. 159.

[3] M'Kendrick. Schäfer, II, 1185.

9.

Luciani[1] objects that "phylogenetically the simultaneous development of the excessively numerous specific differentiations of the fibres of the auditory nerve offers great difficulty." If we were not familiar with any other eye but that of man, should we not possibly make an objection similar to the above in the case of vision? But as we know eyes, varying in development from the simplest to the most complex, the matter causes us no difficulty. Does not the same line of reasoning apply to the ear? The development of the ear can be traced from a primitive apparatus for stabilising purposes, through the depth perceiving mechanism of fish[2], to the sound perceiving mechanism of the frog, the alligator and the bird, to reach its highest development in the bat and ourselves.

10.

Keith[3] states that even on Helmholtz's hypothesis "...the final process of analysis must be done in the brain...because the central exchange has only one key or clue to the particular station or stations from which the messages come: namely the rate at which the stimuli or messages arrive." Keith states on the previous page that "Young had postulated three different kinds of nerves for the retina; von Helmholtz went a considerable degree farther and postulated many thousands of different kinds of nerves for the ear. The most impossible aspect of von Helmholtz's theory comes to light when we carry it beyond the cochlea into the nervous system." In the case of the skin we find numerous sensory end organs with a very high degree of localisation. In the case of the eye also we find that each cone has its own local sign. Is it not possible to suppose that the same thing is true of the cochlea? That just as we learn by experience the relative positions of our fingers and of the cones of our retinas, so similarly we learn the relative positions of the different hair-cells of our cochleae? It is true that we cannot run our fingers up and down our basilar membranes as on the key-board of a piano, and so correlate touch with hearing, but neither can we with our fingers stimulate our retinas. Is it not possible that different combinations of sound waves can educate the ear in the same way that different combinations of light waves teach our eyes? I cannot believe that the central exchange of the brain uses the rate at which the stimuli arrive as the clue in the case of the ear, any more than it does in the case of the eye or the skin.

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[1] Luciani, IV, 236.

[2] Hartridge, *J. of Physiol.* 1920, LIV, 244.

[3] Wrightson, *op. cit.* 160.

11.

Luciani[1] wrote that "Ewald's theory, unlike all other theories (including that of Helmholtz), is based not on purely hypothetical considerations, but on a physical fact which can be experimentally established by a model, which adequately reproduces the fundamental conditions under which the auditory apparatus acts." M'Kendrick[2] did, in point of fact, construct a model of the cochlea on the basis of Helmholtz's theory, and observed the physical facts of sympathetic vibration with it.

12.

Meyer[3] has advanced the objection "that theories which compare the fibres of the basilar membrane to resonators introduce in opposition to the laws of biology the idea of a permanent, constant tension of the basilar membrane. Authors who expect their readers to accept it as the truth should first of all try and convince them of the possibility of living animal tissues retaining their tension for any length of time." The truth of this statement about living tissues is, I think, open to considerable doubt. The heart and skeletal muscles in man are under tension during the greater part of his life. The sphincters of the bladder and anus are firmly closed, and under tension during probably 99 per cent. of the total day. The ligamentum nuchae of the horse (and possibly also of the sheep and ox) supports the weight of the head, not only during waking, but also during sleep. The diaphragm in most animals is in tension during the whole life-time. Lastly, the outer coats of the eye have to stand the intra-ocular tension of *circa* 25 mm. of mercury during life, and do so, moreover, with practically no observable stretch. The conclusion is then, that living tissues DO retain their tension without appreciable weakening for very considerable lengths of time, and, therefore, there is no very obvious reason why the fibres of the basilar membrane should not be able to do so also.

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[1] Luciani, IV, 243.

[2] M'Kendrick in Schäfer's *Text-book of Physiology*, II, 1182.

[3] Meyer, *An introduction to the mechanics of the inner ear*, 1907, 19.

But in this connexion I would direct the reader's attention to Keith's description of the stria vascularis[1] where he asks "is the tension of the basilar membrane regulated by the stria vascularis?" And to this question he replies, "I cannot do otherwise than maintain the opinion" that it does. We know no more at present than Keith has told us of the histology of the mechanism, but it is possible that it will allow the tension to be so far reduced that the basilar fibres can obtain 'rest.' If the tensions of the fibres do in fact change from time to time then the musical tone to which any one resonator responds must also change (a fall in tension of the stria vascularis causing an apparent rise in musical pitch). It may possibly be found that this explains why good singers occasionally sing persistently sharp or flat, and why musical tones fall in pitch during yawning.

13.

"Resonance breaks down if one of the chief factors, viz. the elasticity of the liquid medium between the stapes and the auditory nerves does not at these minute pressures exist."..."It seems difficult to identify resonance which takes time to transmit its energy through air with transmission through a liquid medium which is instantaneous[2]."

Both the above criticisms are due to a misconception of the physical properties of water. In the first place a fluid possesses elasticity whether it be subjected to pressure or not. Thus air conveys sound waves because it possesses mass and elasticity, the fact that the pressures are small (when the sound is a soft one) does not affect the matter. Similarly, the second criticism may, in its turn, be criticised: that in the first place energy transmission through a liquid medium is not instantaneous. Thus sound does not travel through water instantaneously but with a speed of 1435 metres per second, that is only about 4·3 times as fast as through air, and in the second place even if the transmission were instantaneous would resonance be impossible? Surely not! It would only mean that the resonating body would be set into oscillation a little sooner. Wrightson's attempt to prove that liquid is less favourable than air therefore fails, and, as a matter of fact, under certain conditions which are satisfied by the ear, the exact opposite is the case. The physical differences between the two media which concern the problem at hand are as follows: (*a*) compressibilities: with air the volume is halved when the pressure is doubled, with water the volume remains practically unchanged (its compressibility is one twenty-thousandth part that of air; (*b*) with regard to densities: that of water is roughly 777 times that of air.

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[1] Wrightson, *op. cit.* 181.

[2] *Ibid.* 130.

If then the cochlea contained air it would be advantageous to keep the volume small, in order to reduce the compression which would follow change in pressure due to movement of stapes. It would also be advantageous to make the areas of the piston of the stapes and of the basilar membrane as large as possible, in order that a given force being distributed over as large an area as possible, the pressures developed would be kept low and therefore the compression of the air would be kept low. The cochlea would then best take the form of a shallow tube, one end being fitted with the piston of stapes, the basilar membrane stretching down the middle parallel with the piston, the lower end being fitted with the drum of the round window. If, on the other hand, the cochlea contained water, it would again be advantageous to keep the volume small, not in order to reduce compression as in the case of air, but in order to reduce the force required to set the mass of water into motion, because the density (mass per unit volume) of water is so great. The piston of the stapes and the basilar membrane should be given then the smallest dimensions compatible with histological requirements; this would reduce the weight of the parts which have to be moved, and therefore the energy losses due to that cause and will not at the same time involve difficulties due to compression because of its very low values in the case of water. This explains the small dimensions of the organ of Corti in mammals. With regard to the shape of the cochlea, physical considerations show that it confers on the basilar membrane, and therefore on audition almost complete immunity from the effects of changes of position and of both linear and angular accelerations; this immunity is contributed to by the fact that the densities of the organ of Corti and the fluid bathing it are very nearly the same.

The fluid confers two other advantages: (*a*) it makes the volume displacement of the basilar membrane equal to that of the piston of the stapes since the intervening fluid is nearly incompressible and therefore ensures that the basilar membrane shall react to weak sounds; (*b*) it increases the rapidity of damping of the basilar resonators. This latter action may be explained as follows: suppose a set of resonators having been set into oscillation by movements of the stapes piston under the stimulus of sound waves on the drum, and that these sound waves suddenly cease, then if the cochlea was filled with air these resonators could continue in vibration by causing alternate compressions and rarefactions of the air in the scalae. The oscillations would therefore only gradually die out under the natural damping of the vibrators themselves. But if now air be replaced by fluid, no such oscillations could continue, unless either the basilar membrane drives the piston of the stapes in and out, or other parts of the basilar membrane are set into vibration. The former has against it the fact that energy would have to be available for setting the masses of the chain of ossicles and drum into vibration, and the tensions of the stapedius and tensor tympani muscles and friction would have to be overcome, and is therefore unlikely, the latter has against it the fact that these other parts of the basilar membrane have natural periods very different from that of the part which is trying to keep in motion, that is that the vibrations that occurred would be forced ones, which would have a very strong tendency to die out, and would therefore tend to check strongly the 'after' motion of the vibrating resonators. It is possible therefore that the effective damping of the resonators is greater than Helmholtz supposed, owing to this fluid effect. This gives an additional factor to be taken into account when explaining the staccato ending of a musical tone[1].

14.

It will have been observed that the pitch limits assumed as a basis for calculation are those of from 40 to 40,000 vibrations per second (pp. 364, 370).

With regard to the lower limit, although values as low as eight vibrations per second have been given by authorities, it is by no means clear that the prime tone, and not one of its overtones, was stimulating the ear. Helmholtz therefore on the basis of his own experiments, in which the effect of overtones was carefully guarded against, concluded that 40 vibrations per second is a lower limit which is seldom definitely passed.

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[1] Perrett, *Some questions of phonetic theory*, 1919, ch. v, 36. Perrett, *Nature*, Nov. 7, 1918, 185. Hartridge, this *Journal*, 1921, XI, 285.

Helmholtz pointed out further that notes near this lower limit appear to be alike in pitch, and that as the pitch falls so also does their apparent intensity. He pointed out that these facts would fit in with the conclusion that the longest resonator in the cochlea is set into forced vibrations by notes with a pitch considerably lower than that of itself; this action being entirely in agreement with the probable physical properties of these resonators as explained above in section 1. It is not easy to determine the actual natural pitch of the longest resonator, but its value is probably somewhat higher even than 40 vibrations per second.

Concerning the upper audible limit which is variously given as 40,000–50,000 vibrations per second, Myers[1] has shown by experiments, in which the actual pitch was measured physically, that notes reach the audible limit for most young individuals at between 20,000 and 25,000 vibrations per second.

He showed further that the higher values found by other observers were due to reliance on the determination of the pitch of the Galton whistle by means of a formula which he found to be erroneous. He observed also that as the pitches approach the limit they decrease in intensity and appear to become identical in pitch. The shortest resonator would also seem therefore to be set into forced vibrations by notes that have a higher pitch than itself, and the perception of such notes is therefore due to this fact.

It would seem highly probable therefore that while the audible limit lies between 40 and 25,000 vibrations per second, the series of resonators in the cochlea vary by considerably less than this, say from 50 to 15,000, *i.e.* by the ratio of 1 to 300.

At present evidence is wanting as to the exact values for the pitches of the resonators themselves, but if such values as I have just given were ultimately confirmed by experiment, then some readjustment would be required in the calculations that have been given earlier in this paper. As, however, the extremes of pitch, which I have assumed, are 40 and 40,000 (*i.e.* in ratio of 1 to 1000) whereas the more probable values are 50 to 1500 (*i.e.* in the ratio of 1 to 300), the revised values would be still more greatly in favour of the resonance theory.

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[1] *J. of Physiol.* 1902, XXVIII, 417.

15.

SUMMARY.

(1) Further evidence is given in favour of the view that the probable physical properties of the resonators of the organ of Corti can account for the sensation of tones varying from the highest to the lowest.

(2) The fact that the fibres of the basilar membrane are clothed above and below by cells is in keeping with the physical requirements of the resonance hypothesis.

(3) The number of vibrating elements in the organ of Corti is more than sufficient to account for the total number of different pitches found experimentally, even if the 'all-or-none' law is assumed to hold in the case of the cochlear nerve.

(4) An explanation of the more than sufficient number of basilar resonators is given.

(5) The fact that the fibres of the basilar membrane are connected side-ways does not offer difficulty to the resonance hypothesis.

(6) On the contrary, it would greatly increase the efficiency of the resonators if the basilar fibres were connected so as to form a complete membrane.

(7) Even if we assume that the cochlear nerve obeys the 'all-or-none' law, the perception of notes of different degrees of loudness is still readily explicable.

(8) If there was definite histological evidence that two or more hair-cells are fed by one cochlear nerve fibre, it would not invalidate the resonance theory.

(9) The conception of the phylogenetic simultaneous development of the exceedingly numerous specific differentiations of the auditory nerve offers no difficulty to the resonance theory.

(10) That on the resonance hypothesis the final analysis is not done in the brain by the rate at which the stimuli arrive, any more than it is done in this way in the case of touch or vision.

(11) Ewald's theory of hearing is not unique in having been tested by means of a model. The resonance hypothesis was also tested in a similar way by M'Kendrick.

(12) The idea that the existence of a constant tension in the fibres of the basilar membrane is contrary to the laws of biology is clearly erroneous, since many structures (for example in the cornea) are under tension continuously throughout life, without showing signs of giving way.

(13) The resonance theory explains why liquid should be a more suitable medium than air for conveying the sound impulses from the piston of the stapes to the resonators.

(14) Whereas the audible limits of pitch lie between 40 and 25,000 vibrations per second, the free periods of the resonators probably vary by less than this, say from 50 to 15,000 vibrations per second, *i.e.* by the ratio of 1 to 300.

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NOTE ON A METHOD OF ESTIMATING THE TRUE STANFORD-BINET INTELLIGENCE QUOTIENTS OF ADULTS.

By C. A. RICHARDSON.

THE following method of estimating the true Binet (Stanford Revision) Intelligence Quotients (I.Qs.) of adults, or of children approaching 16 years of age, which has occurred to the writer in the course of a general enquiry into methods of estimating the true I.Qs. of adults and adolescents, may perhaps be of interest to those engaged on Binet tests.

The difficulty which occurs in this connexion with the Binet scale lies in the fact that, owing to lack of extensiveness in the upper ranges, I.Qs. of adults, as measured by the scale, are bound to drop below their true values. The greatest possible I.Q. at which an adult can test is about 126.

The method to be described might be called the 'percentile rank (P.R.) method,' for it depends on the fact that the P.R. of a given person, in the order of intelligence, will remain nearly constant. For the method to be used, two things must be known, namely: (1) The distribution of children's Binet I.Qs.; (2) the distribution of the mental ages of adults as measured in the ordinary way by the Binet scale.

The first is fairly well known (cf. Terman, *The Measurement of Intelligence*, pp. 66 and 78), but there are not yet sufficient data to obtain the second accurately, though Terman gives a diagram based on 62 'normal' cases on p. 55 of the book referred to. The writer has, however, found the latter useful as a rough working basis. [N.B. In Terman's diagram on p. 55, 15·11 seems to be a misprint for 16·11.]

To find Binet I.Qs., proceed thus: Having obtained the mental age of any particular adult in the ordinary way, find his P.R. from the distribution table for mental ages of adults. Since, in finding the I.Q., the chronological age is taken for every adult as 16 years, the P.R. obtained will also be the given person's P.R. as regards I.Q. Then from the distribution tables for children's I.Qs., which are true I.Qs. (the distribution remaining the same for different age-groups), it is possible to find directly the approximate value of the given adult's true I.Q., his P.R. having been found as explained above.

In conclusion, two points should be noted, namely: (1) for I.Qs. below about 105, the value obtained in the ordinary way is probably very nearly the true one; and (2) Terman's distribution diagram for adults can only be used up to mental ages of about 18 years 6 months. When more extensive data are available for obtaining this distribution, the percentile rank method should give I.Q. values of considerable accuracy.

Table of Provisional Values obtained by P.R. Method.

Mental age found by usual method	True I.Q.
16 years 6 months	107
16 „ 9 „	108
17 „ 0 „	110
17 „ 3 „	112
17 „ 6 „	115
17 „ 9 „	118
18 „ 0 „	122
18 „ 3 „	126
18 „ 6 „	132

(*Manuscript received* 6 *February* 1922.)

PUBLICATIONS RECENTLY RECEIVED

An Introduction to Psychology. By SUSAN S. BRIERLEY. London: Methuen and Co. 1921. Pp. 152. 6*s.* net.

"This book," Mrs Brierley explains, "has been written to meet the first needs of non-professional students of psychology, and particularly of those who take up the subject in classes organised by the Workers' Educational Association. Its structure is the outcome of several years' discussions with such students." She divides the book into two parts, the first dealing with the scope and method of psychology, the second entering into some of its general problems, the 'wish,' the organism and its environment, instinct and intelligence, the conscious and the unconscious. A useful list of books suggested for further reading is given at the end of the book.

Mrs Brierley's attitude is, as a rule, a fairly temperate one. She is sympathetic to 'behaviourism.' She contends, for instance, that "thought always proceeds in terms either of concrete images of objects and events, of verbal images, of actual movements of the speech apparatus or of all these together. Thought *is not* either images or words, but consists in the use we make of these" (p. 107). Where thought is imageless she believes that it is still accompanied by and dependent on "movements, actual or incipient, of the speech mechanisms" (p. 106). But she insists that "any really adequate psychology must find room both for the facts of behaviour and those of consciousness" (p. 10). She is sympathetic to psychoanalysis, even to the point of accepting without modification Freud's observations on the 'reality' and 'pleasure-pain' principles. But she adopts the term 'horme' as a substitute for 'libido' (in its wider sense), and regards 'dispositions' and 'tendencies' as the "concrete forms," the "springs of action," of the horme. "The horme actually exists in these specific tendencies or 'dispositions' towards specific behaviour" (p. 47). She regards the 'wish' of the psychoanalysts as equivalent to such tendencies or dispositions (p. 52). She speaks of the nutritional horme and the sexual horme.

The special charm of the book lies not merely in its literary style, but also in the clever way in which the beginner is successively led from one to another of the main problems of psychology. There are, however, small blemishes which might well be removed when, as is certain to be the case, a new edition of the book is called for. Simple reaction times do not constitute a good fatigue test (p. 22). Because something exists, it is untrue to say that it can therefore necessarily be measured (p. 23); sensations surely exist, but *quâ* sensations, they are immeasurable. And it is surely unpardonable, in an introduction to psychology, to speak of *sensations* of pleasure (p. 63).

The Psychology of Medicine. By Dr T. W. MITCHELL. London: Methuen and Co. 1921. Pp. viii + 187. 6*s.* net.

Dr Mitchell has written this book "primarily for those readers who have had no professional training in either Medicine or Psychology, but who are anxious to keep themselves abreast of modern thought in these departments of knowledge." The title is not free from ambiguity: the contents are limited to descriptions of hypnosis and hysteria, the theories of dissociation, repression and the unconscious, psychoanalysis, dreams and the neuroses, psychotherapy and the prevention of the neuroses. The book is hence mainly occupied in a presentation of the views of Janet, Freud and Jung. This is very thoughtfully carried out, as would be expected of a writer of Dr Mitchell's experience and repute.

The Boise Survey: a concrete study of the administration of a city school system. By Professor J. B. SEARS assisted by W. M. PROCTOR and J. HAROLD WILLIAMS. World Book Co., Yonkers-on-Hudson, N.Y. 1920. Pp. viii + 290. $2.25.

Boise is a city of about 35,000 inhabitants, the capital of the state of Idaho. Professor Sears, Associate Professor of Education in Leland Stanford Junior University, and his two colleagues were asked in 1919 to survey its public school system. The report deals with the following problems: organization and administration, teaching staff, curriculum, efficiency of instruction, the health, progress and individual differences of school children, buildings and grounds, the high school, educational and vocational guidance, costs and business management. Comparisons are made between about forty other cities in the county and Boise. Tests in spelling, arithmetic and handwriting were given to a large number of children.

State Maintenance for Teachers in Training. By W. S. HERTZOG. Baltimore: Warwick and York. 1921. Pp. 144.

The author surveys the conditions justifying state aid for prospective teachers and he describes the systems which are at work in Europe, our Dominions and elsewhere. He points out that in this respect the United States ranks behind other countries.

The Value of School Supervision. By M. S. PITTMAN. Baltimore: Warwick and York. 1921. Pp. x + 129.

Two groups of schools are here compared, both situated in South Dakota, the one serving as a 'control' group, the other being an 'experimental' group to which the 'zone' plan of supervision was applied. "The special interest of the supervisor is the improvement of certain phases of the school work." The data given go to "show that supervision practically doubled the efficiency of the schools for more than half of the school subjects....School attendance was improved and the children of the advanced grades remained in school longer."

The New Psychology and the Teacher. By Dr H. CRICHTON MILLER. London: Jarrolds. N.D. Pp. 232. 6*s*. net.

The 'new psychology' is of course that of the psychoanalysts, or, as Dr Miller prefers to call them (since he clearly favours the school of Jung), analytical psychologists. There is little that is really new or inaccurate in his presentation of such teaching, but the book makes delightful reading. Its outstanding merit consists in the easy, attractive, really admirable form in which its contents are expressed.

Studies in Human Nature. By Professor J. B. BAILLIE. London: Bell and Sons. 1921. Pp. xii + 296. 15*s*. net.

Professor Baillie shows in these studies that he is dissatisfied with mere logical reasoning as a basis of general and philosophical knowledge. He discusses, mainly from a philosophical standpoint, the place of imagination in knowledge, the function of emotion in the awareness of the reality, the nature of memory knowledge, the sources and significance of laughter and tears, etc., without, however, committing himself to any definite philosophical theory.

Dual Evolution. By J. O'CALLAGHAN. London: Bell and Sons. 1921. Pp. viii + 260. 12*s*. net.

The author endeavours to reconcile idealism and realism from the standpoint of humanism; he admits both a material and a spiritual world.

Reports of the Industrial Fatigue Research Board. No. 12. *Vocational Guidance* (*A Review of the Literature*). By B. Muscio. 1921. Pp. 57. 1*s*. net. No. 14. *Time and Motion Study.* By E. Farmer. 1921. Pp. 63. 2*s*. net. No. 15. *Motion Study in Metal Polishing.* By E. Farmer and R. S. Brooke. 1921. Pp. 65. 2*s*. net. No. 16. *Three Studies in Vocational Selection.* By B. Muscio, A. B. B. Eyre and E. Farmer. 1922. Pp. 86. 1*s*. 6*d*. net. London: H.M. Stationery Office.

These are psychologically by far the most important reports yet published by the Industrial Fatigue Research Board. The first is a valuable *catalogue raisonné* of books, etc. published on vocational 'selection' (the title seems a misnomer). Prof. Muscio gives a careful criticism of the results obtained in clerical and engineering occupations, metallurgy, music, printing, salesmanship, telegraphy, telephone exchange work, etc. Next follows Mr Farmer's report on time and motion study, divided into three parts, of which the first reviews the previous work on the subject, the second describes examples of motion study recently carried out by the author on behalf of the Board, and the third gives a bibliography of over 100 relevant books and papers. Mr Farmer rightly lays greater stress on rhythm than on speed of movement, and he objects to the setting up of a standard rate of work based on time study. He argues that time study should aim rather at initiating lines of improvement. In his report on motion study in metal polishing, where he was assisted by Mr Brooke, Mr Farmer gives striking evidence of the increased output effected by training inefficient or inexperienced workers systematically, and of the wasteful expenditure of energy which occurs towards the end of the day, with diminished output, as fatigue sets in; for the latter demonstration he ingeniously employs a recording watt-meter. The last report shows (i) that a high correlation exists between the ranking of hand compositors according to their technical efficiency and their ranking according to certain tests applied to them by Prof. Muscio, (ii) that Martin's muscular strength test which has been greatly praised in America suffers from serious defects, and that a combination of strength of grip and weight (numerous tables derived from over 2000 adolescent males are given) provides a useful indication of general strength, and (iii) that physical type —as determined mainly by finger span and by finger and hand length and breadth measurements—plays some part, though a small one, in proficiency in the work of a sweet factory.

Die Raumwahrnehmung der Blinden. Von Dr W. Steinberg. München: Reinhardt. 1920. S. viii + 150. M. 3.25.

The work commences with a sketch of the problems raised by the psychology of the blind; this includes a historical review, an account of the chief facts involved in distinguishing two tactile stimulations from one, an analysis of the procedure in
tactile perception of form, a description of the manner in which the blind orient
space, and a consideration of how the loss of vision influences the
onality. Then follows an account of the author's own experiments
men. These had to estimate the form of small wooden prisms
3–6 straight lines. In one part of the experiments,
one or both stationary hands; in the other part,
or of both hands was used to grope along the

the grasping with stationary hands produces
tely, although still inadequate to the objects,
all its features. The groping along the objects,
othing but "movement-shapes," which are
hen, by a further operation are converted
Solely in the case of the born-blind

the body between the shoulders. The limits of the outer portion are given by arm movements; the arms explore this outer zone in a manner resembling that of searchlights. Beyond this zone, the space is only apprehended in a non-intuitive (*unanschaulich*) manner. Walking to a different place furnishes a measure of the distance, but the apprehension remains one of movement, and does not become converted into purely spatial terms.

In this work, the psychological analysis is conducted along the beautiful lines of the younger Austrian school, and it deserves most careful study by any one interested in the tactual perception of space, either from the theoretical or even from the practical standpoint.

The Glands regulating Personality. By LOUIS BERMAR. New York: Macmillan and Co. Pp. 300. 18*s.* net.

This book contains a valuable account of behaviour from the point of view of glandular changes. It deals with facts which are indispensable to the student of modern psychology. But it is spoilt by obscure ontological and teleological views which have but little bearing on the subject, and the attempt to identify the subconscious mind with the vegetative system is worthless. The main thesis, however, is of great interest, and is presented clearly and not too popularly to lack scientific importance.

Psychoanalysis and Behaviour. By ANDRÉ TRIDON. London: Kegan Paul, Trench, Trubner and Co. 1921. Pp. 354. 10*s.* 6*d.* net.

This is a simple popular account of the main doctrines of psychoanalysis, supplemented by four valuable chapters on the distinctive features in the teaching of four well-known psychoanalysts—Freud, Jung, Adler, and Kempf.

The conclusions stated in the main part of the work are, with one or two exceptions, guarded and moderate. The book is free from the wild generalisations which make so many popular works on this subject scientifically worthless, and publicly dangerous.

The title is slightly misleading; although the author has clearly been influenced by the behaviourist teaching of Kempf, it is not clear to what extent he is in accord with the more extreme views of Watson. The book is not an attempt to link Psychoanalysis on to Behaviourism as taught by the last-named writer.

The arrangement of the book is excellent, it is clearly written, and each chapter is supplied with a useful and intelligently chosen bibliography.

Die Psychische Dingwelt. Von Dr WILHELM HAAS. Bonn: Friedrich Cohen. 1921. S. viii + 216. M. 28.

The exigencies of life have forced man to recognise the objective reality of the
physical world, but not of the psychical world. The psychical world, however, is just
as real as the physical world, though different from it. According to Dr H
reality of the psychical world is closely parallel to that of the ph
independent psychical things just as there are independent p
'modes' (*Weisen*) of psychical things are as independent
the qualities of physical things. There are three
(*Gehalt*), or that which is apprehended in acts
or the objective counterpart of processes of fee
the objective counterpart of processes of valu
present in psychical objects, though we do no
neither physical nor psychical, but is intimat
psychical worlds. Some psychical things, lik
are more closely related to the Ego than a
body there is a psychical body; but just as
than its material b

NOTES ON RECENT PERIODICALS.

Psychologische Forschung, Band 1, Heft 1 und 2. August 1921.

Zur Psychologie des Schimpansen (Wolfgang Köhler).

A series of mainly qualitative observations on the chimpanzee made by the author at the *Anthropoidenstation* at Teneriffe. Only two illustrations of these can be here given. The chimpanzee Sultan was confined alone in a lattice-work enclosure, while the experimenter buried a pear deeply in the sand at 140 cms.' distance from the enclosure, covering it up and disturbing the sand in its neighbourhood so that the exact spot of burial was quite lost. Sultan at first showed momentary disappointment while this was done and then turned to play within the enclosure. Six minutes later the experimenter approached Sultan who grasped his hand but was repulsed. When after nine minutes the experimenter again approached, the chimpanzee repeated this behaviour with the evident object of directing the experimenter to a stick which lay to one side of the burial ground and out of reach of his enclosure. The experimenter did not satisfy him, and later (17 minutes after burial) Sultan repeated his behaviour. Half an hour after the burial, while the animal was otherwise occupied, the experimenter laid the stick where it could be grasped only with difficulty from the enclosure. At first Sultan did not perceive this change; but immediately after doing so, he seized the stick and began digging at the right spot until he had unearthed the pear. On the following day, a pear was buried 130 cms. from the lattice work, about two metres to one side of the previous burial place, while Sultan remained quiet, playing contentedly in his enclosure. After an hour, the attendant introduced a stick into the cage, through a window in the back wall. Sultan at once seized it and after an initial error of position, dug up the pear. In another series of experiments, two chimpanzees who had learnt by practice to distinguish between a large and a small rectangle by choosing the former, distinguished between the large and a still larger rectangle by generally choosing the latter; another chimpanzee who had learnt to choose the smaller of a pair of rectangles still chose the smaller (after initial hesitation and choice in the absolute sense) when the larger rectangle was replaced by one smaller than the other one.

Untersuchungen zur Lehre von der Gestalt (Max Wertheimer).

This, the first of a series of special papers on the theory of form, is of an introductory and general character. Parts are not to be considered as being compounded to make up a whole. The form of the whole is not determined merely by its parts. Parts are generally to be treated as members of the whole, the simpler being derived from the more complex, not *vice versâ*.

Tod und Leben bei den Kpelle in Liberia (Dietrich Westermann).

A chapter of Liberian ethnology, describing primitive conceptions of the mind-body relation after death.

Beiträge zur Psychologie der Gestalt (V) (P. Cermak und K. Koffka).

This is the fifth of a series of articles of which the first four have already been published by Prof. Koffka in book form after their appearance in the *Zeitschrift für Psychologie*. It deals with the visual sensations of movement in interrupted and continuous illumination, and sets out to determine the wider scope of the laws originally deduced by Korte for the relation of the temporal, spatial and intensity characters of the stimuli to apparent movements, *i.e.* to those phenomena of movement which are produced by simple successive stationary stimuli.

or of one's environment, we see such other psychical things. The problem of the nature of the Ego and its relation to the psychical and the physical, and the problem of the relation of the psychical and the physical to one another, are to be dealt with in another volume.

Vom Wesen des Bewusstseins. Von Dr LUDWIG KLAGES. Leipzig: Johann Ambrosius Barth. 1921. S. vi + 94. M. 12.

This is an expansion of a lecture on the biological bases of consciousness. The author only sketches the outlines of a comprehensive Psychology and Philosophy which he hopes to publish in the near future. Some of his ideas Dr Klages has already published in the *Deutsche Psychologie* under the title of "Geist und Seele."

An Analysis of Certain Theories of Truth. By GEORGE BOAS. (University of California Publications in Philosophy, Vol. II, No. 6, pp. 187–290.) Berkeley: University of California Press. 1921. $1.25.

The theories examined are: Subjective Theories (including (*a*) Logical Hedonism, and (*b*) Truth as the Irresistible), Relational Theories (including (*a*) the Correspondence Theory, (*b*) Truth as Formal Consistency, (*c*) Coherence in a Significant Whole) and Voluntaristic Theories of Truth.

Über optische Inversion (Erich M. v. Hornbostel).

This deals with illusions of reversible perspective, introducing several novel instances of the phenomenon. It is curiously written in a form directed to the reader who is throughout addressed as *du*.

Eine Pseudofovea bei Hemianopikern (Wilhelm Fuchs).

This paper opens with a brief summary of the writer's previous work on the subject. It contains detailed observations on a single patient whose condition of right-sided homonymous hemianopia was due to a shrapnel wound of the skull. The writer shows how the pseudofovea or new region of distinctest vision develops within the remaining field of vision, and how its position varies with the recognised size of the object, not with the actual distance of one and the same object from the patient. The larger the size of the object, the more peripheral becomes the pseudofovea. When a letter, previously falling on the pseudofovea, is displaced so that it falls more and more outwardly on the retina, it becomes increasingly blurred and indistinct. If displaced inwardly, it may merely lose its form and meaning. This occurs not merely because its size is unfitted for that particular region of the fovea, but also because its form or meaning is unfitted. A number of separate short lines covering an appropriate extent can be recognised as a distinct and significant whole, whereas when only a single short line falls on the same retinal area it is blurred and indistinct; and when only some of a number of short lines can be apprehended as a form, only these are distinct while the remainder are blurred and of lighter colour.

The Psychological Review (Vol. XXVIII).

No. 1. *January* 1921.

The Truly Psychological Behaviourism (Mary Whiton Calkins).

Extreme behaviourism, denying or ignoring consciousness, is not psychological at all. Modified behaviourism, treating consciousness as a distinctive function of the organism, too readily falls into the extreme form. The truly psychological behaviourism is a self-psychology which emphasizes the study of the relation of self to environment.

An Attempt towards a Naturalistic Description of the Emotions (J. R. Kantor).

An elaborate paper mainly intended to show that emotions are forms of behaviour which mark the absence of any definite response system. They are chaotic responses to unusual features of the environment.

[illegible] *Tone of Lines* (Helge Lundholm).

[illegible] study. There is a feeling-tone connected with "pure lines." This [illegible] the suggestion of movement. Emotions with little motor [illegible] with long and low waves; emotions with strong [illegible] waves of acute angle.

[illegible]

[illegible] go two phases of development. In [illegible] dered enjoyable; in the second [illegible] ulum swing from extreme

[illegible]

No. 2. *March* 1921.

Cerebral-Mental Relations (Shepherd Ivory Franz).

There is a general dependence of mental states upon the state of the brain, but no special dependence of a special mental state upon the integrity of special cerebral parts.

The Misuse of Instinct in the Social Sciences (L. L. Bernard).

Social "instincts" are aggregates of habits, and in no sense fixed and simple responses. It is not true that for social purposes instinct is dominant over habit, and both over the influence of the environment.

An Attempt towards a Naturalistic Description of the Emotions (II) (J. R. Kantor).

Emotions are of no particular biological utility, cannot readily be classified, and "are seldom if ever found in animals and young children."

On the Organisation of Intellect (Edward L. Thorndike).

An application of Spearman's 'test' of the truth of his 'General Factor' theory to data obtained by applying fifteen tests of intelligence to about 800 soldiers, and seven tests of intelligence and of 'intelligence mixed with skill' to over 900 soldiers. The enquiry suggests that the organization of intellect involves processes much more complex than those required by Spearman's theory.

No. 3. *May* 1921.

In Memory of Wilhelm Wundt (by his American students).

A series of short appreciations by various American psychologists who studied under Wundt.

The Structure of Animal Learning (J. A. Melrose).

In a very long paper it is maintained that lower animal learning involves (1) organic fixation, (2) organic spatial adaptation, (3) organic choice, and higher animal learning comprises (1) organic association, (2) organic conception, (3) organic judgment.

A poet's portrayal of Emotion (Frances Theresa Russell).

A study of certain of Browning's descriptions of emotions.

No. 4. *July* 1921.

Dynamic Psychology and the Problem of Motivation (Horace Bidwell English).

Neither hedonism nor McDougall's instinct theory is broad enough to explain
human motives. We must include all relatively permanent psycho-physical dis
among the mechanisms capable of furnishing the impetus to actio

Psychology and the Central Nervous System (Howard C.

Psychology deals with all phenomena "co
environment and organism that belong to t
nomena may be studied by self-observa
neurological methods. No one of these

Apperceptive Abilities (Augusta F

Apperception is a distin
ability, which may

The Control of Another Person by Obscure Signs (G. M. Stratton).

A very interesting study of a case of 'muscle reading' without contact, from which it appears that the subject was, without conscious intention, using visual cues of an exceedingly obscure nature, given with like absence of intention by his guides.

No. 5. *September* 1921.

Belief as a Derived Emotion (W. McDougall).

Belief is essentially the same as confidence, but whereas the latter indicates a striving for some particular end untroubled by any anticipation of failure, the whole process taking place on a practical level, in the case of belief "the striving proceeds in a similar untroubled fashion on the purely intellectual plane." Belief and doubt are related as are confidence and anxiety.

How do we acquire our Basic Reactions (J. R. Kantor).

All psychological processes involve "the two phases of stimulus and response." Tendencies, capabilities and the like, in so far as they are supposed to represent forces inherent in the organism, are mere fictions. An attempt is made to indicate the main classes of conditioning stimuli, to show how stimuli operate, and to discuss certain general types of reaction systems.

Fact and Inference in Raymond Wheeler's Doctrine of Will and Self Activity (Mary Whiton Calkins).

As a result of his experiments on the process of choosing Wheeler concluded: (1) that volition consists in kinaesthetic sensations, and (2) that volition cannot be described as self-activity. The present paper undertakes a study of the introspections of Wheeler's observers, and as a result concludes that his interpretations are unjustifiable.

The Correlation between Interests and Abilities in College Courses (Edward L. Thorndike).

The data examined seem to corroborate the doctrine of a very close relation between the order of an individual's interests and the order of his abilities.

A Device for Determining Coefficients of Partial Correlation (Clark L. Hull).

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETINGS.

January 28, 1922.	Experiments on Memory in Mentally Defective Children. By Lucy G. Fildes. Psychology and Primitive Culture. By F. C. Bartlett.
March 11, 1922.	An Investigation into the Time-sense of Children. By M. Sturt and E. C. Oakden.

SECTIONAL MEETINGS.

Education Section.

December 13, 1921.	The Theories of Freud and Jung in Relation to the Problems of Education. By Constance E. Young.
January 2, 1922.	Mental Tests and Mentality. By T. H. Pear.
February 14, 1922.	Some Experiments on Form-Orientation with Especial Reference to Mirror Writing. By Lucy G. Fildes.

Industrial Section.

December 21, 1921.	Motor Capacity. By B. Muscio.

Medical Section.

December 14, 1921.	The Evolution of the War Neuroses. By Gerald Fitzgerald and James Young.
January 25, 1922.	Homosexuality and Alcoholism. By Robert M. Riggall.
February 22, 1922.	The Constituents of the Unconscious. By Leonard Williams.

JOINT MEETING.

General, Medical, and Educational Sections.

March 14, 1922.	Some Problems of Adolescence. By Ernest Jones.

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